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The ADVANCEMENT OF SCIENCE

Vol. XI, No. 41

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Science and the Unpredictable
Afforestation of Sand Dunes
The Continental Shelf
New Advances in Underwater Observations
Sedimentary Rocks and Tidal Deposits
How Animals find their way about
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THE ADVANCEMENT OF SCIENCE

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THE PROVINCIAL STOCK EXCHANGES[1]

by

G. CLAYTON

I MUST crave your indulgence at the outset by disappointing any expectations you may entertain of learning of the results of a significant advance in economic knowledge. I have set myself the very limited objective of considering the more important economic aspects of the work of the stock exchanges, with especial reference to the Liverpool Exchange.

Let me begin by giving a brief history of the Liverpool market followed by a description of its present organisation. The Liverpool Exchange was founded in 1836 during a period when the spectacular growth of railways had stimulated a new interest in investment. In fact out of a total of 72 securities quoted in a Liverpool price circular of 1836, as many as 39 are the stocks of railway companies. By this time Liverpool's place as the premier market for cotton was firmly established, and Liverpool merchants, in addition to their active trade with North and South America and the West Indies, had entered with characteristic enterprise into the Far Eastern trade as soon as the old trade monopolies in India and China were abandoned.

The founders of the Exchange, eleven in number, represented other interests as well as sharebroking. On the evidence of the directories, Mr. Thomas Barker, Mr. George Batley, Mr. Richard Crewe and Mr. Thomas Forsyth could already describe themselves simply as shareholders, but the remaining members were connected variously with insurance, accountancy, shipbroking and even with activities ostensibly far removed from the business of dealing in stocks and shares. Mr. John C. Da Costa was a wine merchant and Portuguese Vice-Consul. Mr. Richard Dawson played the part not only of a stock and share broker but also of a merchant, gun-powder agent and agent to the Manchester Fire and Life Offices. Mr. Thomas Coglan's interests as 'auctioneer, share agent, general broker and proprietor of the Floating Bath,' were no less widely scattered. Mr. Thomas Hampson and Mr. George Highfield were shipbrokers; Mr. Thomas Harris was an agent to the United Kingdom Life Assurance Company and Mr. Samuel Healey an accountant.

By no means all the shareholders in the City joined the new Association, and in 1843 a rival body established itself under the title of 'Liverpool New Stock Exchange,' occupying rooms in the basement of the Royal Bank Buildings. It was soon clear that two organisations were unnecessary, and the bodies were amalgamated in January 1844. Four years of excited business followed as the great mania of speculation in railway shares grew to its climax in 1847. So extensive became the dealings in the shares at Liverpool that the number of stockbrokers increased by leaps and bounds. Whereas they numbered only 53 in 1843, they rose to 90 in 1845, and to 187 before the crash came in 1847. Surprisingly enough, although the failures included three banks and many merchant houses, only one sharebroker went bankrupt.

There followed a period of steady growth, and in 1890 Mr. Edward Rae, then the Chairman of the Liverpool Stock Exchange, took the initiative in forming the Council of Associated Stock Exchanges, to co-ordinate the policy of the various provincial markets. The same gentleman took a prominent part in initiating legislation, which was finally embodied in the Forged Transfers Acts of 1891 and 1892, to protect shareholders from the dangers of firms repudiating their stock certificates. This was at a time when the London Stock Exchange declined to take any

[1] Paper delivered to Section F (Economics) at Liverpool on Thursday, September 3, 1953.

action until public companies had accepted the principle of indefeasible titles to securities.

Meanwhile the scope of the market's dealings continued to grow especially in rubber, electrical and, with the invention of the internal combustion engine, oil shares. The first decade of the century was so prosperous and trouble-free that it was scarcely to be expected that the market would shortly endure a period of continual doubt and uncertainty as to its future, a period which was notable for two disastrous wars and a depression of unprecedented severity.

The vicissitudes which have beset the exchange since 1914 are reflected in its membership. After the end of the war membership increased to a peak of 157 in 1921. There followed a minor decline and recovery in 1924 in which year the figure stood at 164. From 1929 the decline was progressive until 1934 the low figure of 134 was reached. Thereafter there was a gradual increase until 1938 when the membership was 161 ; the 1939–45 war engendered another slump in membership and in 1946 it had fallen to 116. There was a post-war recovery until 1949 (membership 135) since when there has been a steady decline.

The present membership of 124, however, still ensures Liverpool's position as the largest English exchange outside London (Birmingham membership is 102, Manchester 109).

As in London the Stock Exchange Committee have complete jurisdiction over all members and, especially since the war, have exercised great control, through their powers to grant or withhold permissions to deal, over the quality of the companies in whose shares transaction take place. The conditions of membership are that new members must be recommended by two existing members and that they must provide a financial guarantee for a period of four years. The initial expenses involved are Entrance Fee £700—with certain exceptions ; a Share in the General Fund which at the moment is approximately £172 ; a Subscription of £55 and Security of £2,500 which is deposited with the Committee for four years from the date of membership or partnership.

Of the present membership approximately one-sixth are shunters and the remainder are ordinary brokers, for there are no jobbers in a provincial market.

II

So much for the facts and figures : let us now consider the mechanism of the market. There are structural differences between the London Stock Exchange and the provincial exchanges, which are reflected in the methods of operation. As an illustration of these differences, I propose to give brief descriptions both of a typical transaction on the London market and of one on a provincial market.

First let us take the London market and suppose in the manner of 'Happy Families' that we are concerned with a transaction involving Mrs. Pessimist the Seller, Mr. Debenture the Broker, and Mr. Pokerface the Jobber. Mrs. Pessimist, feeling dyspeptic after a poor breakfast at her Harrogate hotel and convinced, after reading in the *Daily Record* that Harry Pollitt is going to contest Bournemouth at the next election and that Labour has just won a by-election at Cheltenham, that Red Ruin is imminent instructs her London Broker, Mr. Debenture, to sell 1,000 Beechams at best. At 10 A.M. Mr. Debenture strolls over to the Exchange and approaches Mr. Pokerface and several other jobbers who deal in Beechams, and asks them what price they are making this morning. He receives various double-price quotations of which the best from his point of view is Mr. Pokerface's quotation of $94\frac{7}{8}$ to $95\frac{3}{8}$: for the uninitiated—94/10½*d.* to 95/4½*d.* If Mr. Debenture considers that this is as good a price as he is likely to get in the existing state of the market, he will probably decide to sell at 94/10½*d.* rather than risk a subsequent alteration in the price.

Once the deal is closed Mr. Pokerface, the Jobber, is now a bull of 1,000 Beecham shares, which he hopes to sell at a better price than he gave for them. During the day many other Brokers may sell Beechams to him to such an extent that, in order to safeguard his limited resources, he will lower his price and instead of quoting

94⅞ to 95⅜, he will first quote 94¾ to 95¼, then 94⅝ to 95⅛ and then 94½ to 95. Possibly at this price sellers will be discouraged and the market will stabilise. But if he senses that the market is now a buyer, he will start to raise his prices. Above all, he and the other jobbers are the price-makers in the different sections of the market. This will serve to show very briefly the essential features of the jobbing mechanism on the London market whereby buffer stocks of securities are used by the jobbers to offset the initial disturbing effects of buying and selling waves, according to their view of the market. But above all, the jobbers make the market —to use the jargon—in the different types of securities.

I have deliberately refreshed your memories of the London jobbing mechanism because it is London's most distinctive feature and is not found in other exchanges such as New York, Birmingham, Glasgow, and Liverpool. But to return to our illustration, let us suppose that Mrs. Pessimist has an old family connection with a Liverpool stockbroker. And, as before, let us suppose that she instructs him to sell 1,000 Beechams at best. Mr. Equity, having first glanced at the financial press to discover the closing price of Beechams on the previous day, strolls over to the Liverpool Exchange at approximately 10.30, when it opens for dealing (note that this is half an hour later than London). On entering the House he will probably go over to one of the 'shunters' and ask him the current price for Beechams. Before the opening of the Exchange the shunter will have been on to London by means of his direct line and will know the average London price. For example he may quote '94/6*d.* to 95*s.*' to Mr. Equity, who may offer to sell at the former. The Shunter will then reveal whether he is a buyer or a seller. Let us suppose that in this case he admits to being a seller also but informs Mr. Equity that *Y*, another broker, will probably buy at 94/9*d.* He will then probably seek out *Y* to close the deal.

These two examples of the methods whereby the same transaction would be handled in the two markets underline the essential differences, which derive from the different functions of the jobber and the shunter. They both provide a service in that their price-making activities serve as a basis for the market's dealings. But the similarity ends there. The jobber is a wholesaler carrying stocks of securities, has no contact with the general public, and relies for his profit on his 'turn.' The shunter is not a wholesaler, but like other brokers, acts for his own clients and charges commissions to gain his livelihood, but in addition acts as a contact between markets. His role is a combination of the 'specialist' in the New York market, who matches buying and selling orders as best he can, and the *arbitrager* in foreign exchange markets who buys in one market to sell at a profit in another. It is the shunter who helps to make the London Exchange and all the provincial exchanges into a national market by eliminating price discrepancies as quickly as possible. And he shunts securities both ways. For example, if there is heavy selling of Cunards in Liverpool while the market in them is inactive in London and elsewhere, the wise 'shunter' will start to take in Cunards in Liverpool to dispose of them in London and elsewhere. The impact of these transactions on London will be felt by the shipping jobbers who will initially take in the Cunards that have been sold in Liverpool at or near the opening price. But if the selling persists in Liverpool and is not matched by buying elsewhere, the shipping jobbers will judge that on balance the market is bearish and will adjust their prices downwards until they are stabilised. Thus the London jobbing mechanism is made available to the Liverpool brokers through the shunters. Of course it is directly available, if the Liverpool brokers choose to deal direct with London as they not infrequently do. In fact it is estimated that approximately one-half to two-thirds of the volume of business between Liverpool and other markets bypasses the 'shunters,' often when the brokers have in the first instance approached them to discover the approximate dealing prices. Such actions are not resented by the shunters who accept the fact that they are providing a service to the market without any guarantee that all inter-market dealings will be exclusively handled by them.

A further point should be mentioned. From the foregoing it might be assumed that London is the price-leader in all types of securities. In fact there are one or two surprising exceptions. Liverpool is still the acknowledged expert in the insurance market and certain sections of the shipping market, Manchester in textiles, Glasgow in ' distillers ' and Edinburgh in the stocks of the Scottish investment trusts. In the case of these stocks the ' shunters ' give the lead from their own respective markets to the others.

Such are the chief differences between the provincial markets and London in performing the function of making a market in old money. In carrying out the secondary function of assisting in the provision of new capital for industrial and other borrowers, the differences are negligible. All the Stock Exchanges under the 1948 Companies Act are assigned specific roles in implementing the requirements of the law itself. For the Act demands that, if it is stated in a prospectus that application for permission to deal will be made to the Stock Exchanges, and if that application is either not made or is refused, then all subscriptions to the issue must be refunded in full. This means, in effect, that it is impossible to make an issue to the public without first submitting it to a searching examination by the skilled and experienced officials of the Stock Exchange, in order to ensure that the possibility of the rejection of an application is reduced to the absolute minimum. In the majority of cases the onus of protecting the public falls on the London Stock Exchange, but when an application for permission to deal is confined to one of the provincial exchanges, it falls on the local Stock Exchange Council. The procedure is exactly the same. The particulars of the company, its profit record over ten years, the method of issue, the arrangements made to secure a free market, and the reasonable level of the profits to be made by participants or vendors, are all vetted by the Committee on Quotations. The only difference between the London and the local procedure relates to cost. In the case of Liverpool the fee for a quotation is 25 per cent. of the London figure and is a maximum of 125 guineas ; the advertising costs are also significantly cheaper in that a local issue only requires an insertion in the Liverpool *Daily Post* at a fee of 100 guineas as compared with one in the *Financial Times* costing 500 guineas. Such a difference in the fixed costs, however, may be an important consideration for a small company going to the public for the first time, a point which I hope to elaborate later. But there are no important differences in procedure.

III

I now wish to attempt an evaluation of the economic significance of the typical provincial exchange in the mixed economy. An essential preliminary is an investigation of the far-reaching changes, which have occurred in the British Capital Market since 1939 and have completely altered the environment in which the Stock Exchanges function.

Let us consider first the general changes which have had their impact on all the various institutions of the Capital Market without exception, and secondly the particular changes, whose adverse effects have been confined to the Provincial Exchanges.

The chief factors have been emphasised on many occasions, but I hope you will bear with me if I enumerate them once again. The most fundamental influence of all is the long-sustained and recently intensified egalitarian fiscal policy which more than any other single factor has shifted the power to save from the wealthy few, to serve whom the present structure of the Stock Exchange was created, to the many. Our data on this subject is inadequate to say the least. However, an examination of Table 31 of the 1951 National Income Blue Book—the most reliable source available to us—clearly reveals that personal saving since 1945, including savings through the important institutions, like life insurance, and building societies, and by farmers and other non-corporate businesses, has scarcely been sufficient to cover death duties and other taxes on capital payable by persons, so that any capital formation by this sector has had to be financed from outside. Prof. Paish, in his address on ' Open and repressed monetary inflation ' (*Ad-*

vancement of Science, No. 38, pp. 169–80), has drawn attention to the magnitude of the Personal Sector's absorption of finance from elsewhere, which seems to have fluctuated from a maximum of nearly £350 million in 1948 to a minimum of rather under £200 million in 1950. Only in 1952 is the Personal Sector estimated to have had a small margin of funds available over those invested, and this estimate depends on a remarkable jump of £140 million in gross personal savings: an item which it is admitted is one of the least reliable in the National Income figures.

Taxation has had important effects on the demand for funds as well as the supply. Although our data is meagre, the results of such investigations as the Federation of British Industries' case-study of eighty firms show that the amount of undistributed profits put to reserve in the last few years has been inadequate to maintain industry's capital over the years, even though the proportion of distributed profits has not risen but fallen. As Dr. R. F. Henderson has shown, the comparatively high level of taxation of corporate income has particularly affected small companies. For they rely much more heavily than large ones on internal profits for the finance of growth, in that they have great difficulty in raising long-term capital from external sources and have to pay dearly for medium-term credit. In fact most companies are forced to resort to some form of external finance at a much earlier stage in their growth than they did before the war. Thus high levels of taxation have increased the demand by companies in the private sector for assistance through the mechanism of the Stock Exchange but at the same time have seriously reduced the supply of risk-bearing funds.

The decline of Britain as a creditor nation and the post-war policies of expansion of the public sector have had similar impacts on the Stock Exchanges. Many hundreds of millions of foreign securities have been repatriated by former debtors of Britain and have been removed from the Stock Exchange list. And although in some cases gilt-edged securities have been substituted, they have not been an adequate compensation in terms of market activity. For example, the Argentine rail stocks were replaced by Treasury borrowings on floating debt from the banks. In addition, nationalisation has involved a large-scale substitution of gilt-edged stock for a vast heterogeneous collection of railway, coal, electricity and gas shares. The consequence for the Stock Exchanges is that gilt-edged form an ever-growing proportion of the available securities—at the expense of industrials, the money value of which is not much more than before the war. This means that the real volume, measured in terms of the cost of doing business, has been approximately halved. And even if it were politically and socially desirable, there remains no new field for future development which can compare with the large areas of the national economy which have been or are being embraced by the public sector. (This is not incompatible with the previous argument that within the private sector the demand for external finance through the Stock Exchanges is likely to be intensified, for it merely means that any significant recovery of the ground lost to the public sector by the private sector is most improbable.)

Of these two fundamental influences, the growth of the public sector and the fragmentation of savings, the latter has had the more serious impact on the mechanism of the Stock Exchanges. Although it is possible to foresee some alleviation of the present burdens of taxation, any realistic evaluation of the future role of the Stock Exchanges must assume that there will be no significant reversal of egalitarian fiscal policies and that the transfer of the power to save from the few to the many will be permanent. Such diffusion of the power to save means that savings will be directed along channels which bring very little direct business to the Stock Exchanges, for example to the Government through the purchase of Savings Certificates and deposits in Savings Banks; or to the chief institutional investors by entrusting deposits to the Building Societies; or through the purchase of life-insurance and annuities. A progressive substitution of small direct investors for the substantial private investors, whose investments and speculations

formerly constituted the average broker's most remunerative type of business, is taking place. Even if the total supply of funds forthcoming were to be maintained, the consequences for the stockbroker would be serious, for however many thousands of small direct investors there may be, most of them cannot provide the type of business which can be economically handled. The more small clients a broker has, the more obvious becomes the discrepancy between the few shillings net each bargain yields and its multifold costs in clerical, administrative and executive costs. Furthermore, so far as may be concluded at present, the business of the small investor, even if it could be made economic, is usually inactive business.

The same factors which are reducing the size of the average individual investor and speculator are reducing the resources of the market itself. Even more clearly than in other professions where partnership is the predominant organisation, neither brokers nor jobbers are able to build up sufficient resources to provide an adequate margin for risk. For the broker, one of the chief results is to widen the gap between the majority of firms and the few large ones, which concentrate on institutional business. The latter are able to achieve certain economies of scale—such as the use of their own statistical departments—and have not suffered through the increased predominance of gilt-edged securities in the market, which is the source of much difficulty for the majority of brokers. For their clients are private investors who, if they invest in gilt-edged, have no tendency to indulge in the switching operations which are frequently undertaken by the large institutions but usually abide by their original preferences. Frequently their initial investments yield insufficient commission to cover the cost of the transaction, but it would be totally uneconomic for the brokers to propose and undertake switching operations for clients who were able to invest no more than two or three hundred pounds.

For the jobbers, the burden of taxation has had even more serious consequences. If they are members of partnerships they suffer income tax and surtax according to the profits earned by their firms. Losses, on the other hand, must be paid out of capital. Consequently jobbing partnerships have found it almost impossible to accumulate capital; usually they have had to accept a reduction in their resources, when older partners have retired and withdrawn their personal capital. While the jobbers' resources have been contracting, the monetary value of securities has greatly expanded. It is not surprising, therefore, that jobbers have felt obliged to reduce the size of the stocks they are prepared to carry and have been less willing to run risks.

Such have been the effects on the Stock Exchanges of the long-term changes in the economic environment. Their difficulties have undoubtedly been aggravated by some other restrictions, which are not necessarily as permanent, such as the 2 per cent. stamp duty and the tax on distributed profits.

All these influences have adversely affected the Provincial Exchanges as well as the London Market, and in some instances to a greater degree. The shift towards gilt-edged has greatly reduced the profitability of provincial stockbroking, for very few provincial brokers have access to the institutional business. The local broker, dealing with the gilt-edged securities of the small private investor, is competing with the branch banks, which at least offer the investor some saving of trouble in handling documents and money. In such business, the provincial broker tends to be at a disadvantage compared with the London broker, who, even if he has to split the commission with a bank or provincial broker, can at least effect the transaction by walking into the market instead of transmitting it over a trunk line or through a 'shunter' whose services must be paid for. In fact it is possible that the 'shunter' or inter-market dealer may become an anachronistic survival in a national stock-market structure, in which dealings in gilt-edged are predominant. The large firms of shunters were built up to serve the speculative investor and performed a useful and profitable arbitrage service by restraining the deviations between dealing prices established in the various provincial centres and in London in the course of extremely active dealings.

The scope for such activities in relation to the present small dealings in gilt-edged or even in such outstanding ordinary shares as Dunlops or Imperial Chemicals is limited.

The severity of the impact of these influences depends to a large extent upon the level of stockbroking costs, for the provincial brokers can overcome many of their disadvantages if their costs are significantly lower than London's. It is difficult to secure any reliable information on this point, but there seems to be a general belief that costs in the provinces are £1 or £2 lower than in London. Nevertheless there are investors in the provinces who believe it to be more profitable to transact their business through a London broker despite the heavy telephone costs.

IV

In the face of such important long-term influences, how should the Stock Exchanges adapt their organisations and methods to fulfil their roles in the mixed economy? We must begin with the assumption that, even with a considerable extension of state ownership, the bulk of industry and commerce will continue to rely for its growth on private finance over and above that which it can raise from internal sources such as undistributed profits: for there are over a million private commercial establishments in this country. Granted, then, that Stock Exchanges in some form will continue to be essential to the economy, in what type of environment will they be likely to operate?

Since the war the policy of successive Chancellors of the Exchequer has been to depend on forced savings through budgetary surpluses to finance a high rate of capital formation without excessive inflation. That such forced and involuntary savings have been transferred into finance and risk capital for private enterprise has been due largely to *certain fortuitous circumstances*, namely, the legacy of wartime finance and to the secondary effects of nationalisations. When an industry is nationalised its shares are withdrawn from the market and exchanged for a gilt-edged security; there is, therefore, a reduction in the proportion of risk-bearing securities held by the general public. At the end of the war, however, the public already held gilt-edged securities in amounts unusually large in relation to the available volume of risk-bearing securities, while the banks had ample holdings of Treasury Deposit Receipts and Treasury Bills. The public, therefore, naturally sought to divest itself of fixed-interest securities in order to restore the composition of its investment portfolios to something more like pre-war levels while the banks wished to see some replacement of Treasury Deposit Receipts and Treasury Bills with more remunerative assets. It was these circumstances which provided the Government with the opportunity for pumping the forced savings in its hand back into the economic system. In the first place the Budget surplus was apparently used for the reduction of floating debt held by the banks, and since the banks at the time were extremely liquid they were enabled to replace Treasury Bill holdings with longer dated gilt-edged securities, and also to extend their direct advances to industry. These advances were in a sense a direct contribution to the nation's supply of risk capital, inasmuch as by enabling businesses to hold higher stocks and to finance book debts and work-in-progress on borrowed money they released part of industry's own resources for expansion. The bank's purchases of gilt-edged securities from the public gave private investors cash resources for reinvestment in either existing stocks and shares or new market issues. To this extent, therefore, the urge to unload the nationalisation stocks and restore risk holdings to previous proportions could be satisfied without bringing upward pressure to bear on the rate of interest. In the second place, the surplus on the National Insurance Funds was used directly for purchasing gilt-edged securities, thus from another source again putting the public in funds for reinvestment in industry.

But there are limits beyond which it is difficult to extend this policy. Some time ago it became an object of monetary policy to curb the expansion of bank advances, and in any case there is reason to believe that many firms have no desire to expand their overdrafts until they have

first increased their share capital. In other words they will cease to borrow until sufficient undistributed profits have been accumulated, or they have appealed to the public for new money through the New Issue market. But the present level of taxation makes it difficult to expand by the former method. Thus we may conclude that the mechanism of transferring forced savings into risk capital is liable to encounter serious difficulties and in the near future will involve inducing the general public to hold proportionately less in the form of ' safe ' investments than was the case before 1939.[1]

I have digressed to consider the implications of the forced-saving mechanism for two reasons. First I wished to indicate the dangers of believing that forced savings through the budget can offset the difficulties of voluntary saving and that, therefore, the Stock Exchanges, as a mechanism for canalising savings can be treated as nineteenth-century anachronisms. Secondly, the above analysis points to a possible line of development by following which the Stock Exchanges may achieve their own salvation and at the same time be of service to the economy. The essential need is to devise ways and means of drawing small business economically from ever-widening circles and improving the machinery of the Stock Exchanges in order to transact business more cheaply. But to adapt the present structure which was evolved to serve the needs of a wealthy minority, so as to enable it to deal, primarily and economically, with the needs of the many, requires radical measures.

A policy of serving the small investor means either increased contacts with small investors or a greater reliance upon the services of business collecting agencies of which the banks are easily the most important for they are in the best position to encourage small business and are the best equipped for dealing with the purely mechanical aspects of payments and deliveries. As many of you know, Mr. J. B. Kinross, the General Manager of the Industrial Finance Corporation has devised a scheme to sell industrial shares in penny units across the counters of the banks. In essence it involves the promotion by the banks and the Stock Exchange of a non-profit making trust company, which would select a list of equities and buy the shares through the market in the ordinary way. The trust would then split each share into units with a nominal value of one penny which could then be bought and sold through the branches of the banks. But whether collaboration took this form or not, the need for closer relations between the financial institutions is obvious, even if at the initial stage it were limited to the provision of more extensive investment services by brokers for the use of branch bank managers.

An essential prerequisite of this policy is the application of a modern clearing house technique to the mechanics of Stock Exchange book-keeping and settlements. Under the present system each bargain gives rise to a whole series of parallel operations in different stockbrokers' offices, and there is constant traffic in securities, documents and cheques. All these operations could be done centrally under one roof and, with modern accounting methods, by one-tenth of the present staffs in one-tenth of the space. It is estimated that overhead costs might be reduced by as much as two-thirds by the adoption of such a system.

Such reforms must inevitably lead to a reduction in the numbers engaged in the stockbroking profession throughout the country—a reduction that may be achieved by the gradual elimination of surplus parts of the organisation and the amalgamation of others, or, on the other hand, by methods which must be in effect aimed at economy of manpower. The question of immediate interest to us is whether the Provincial Exchanges are surplus parts of the organisation and, therefore, condemned to gradual extinction.

It is certain that, while the case for the continued existence of some national market for the exchange of property-rights can be considered proven, the necessity for provincial exchanges given the present preponderance of gilt-edged and ' first class ' industrial shares which command a national market, is less obvious.

In so far as a considerable proportion

[1] On this point see J. R. Cuthbertson's article in *District Bank Review*, 1952.

of current savings only reach the Stock Exchanges through the institutional investors, the prospect for the provinces is not encouraging. The vast majority of the institutions have their head offices in London and transmit their new money through the few large London stock-broking firms which have specialised in such business ; and there is no possibility of any provincial firms being able to compete with them.

But, although it may appear paradoxical, it is my personal view that one of the provincial exchanges' best hopes of survival is to collaborate in the formation of new regional institutions. There is a need for more local issuing houses to sponsor local issues with the support of regional investment institutions, which might draw their funds like Industrial Finance Corporation and Finance for Industry Corporation from the banks and the insurance companies. The creation of such consortia can be justified on various grounds. First, the selection of suitable investment projects involves a high degree of individual judgment and the creation of additional institutions to Industrial Finance Corporation would provide more checks and balances against bad decisions. (The possibility of losing a good investment project would be reduced if it were scrutinised by five or six investment experts before being turned down.) Secondly, it is axiomatic that of all the criteria adopted in determining the suitability of any project the quality of the management of the company is considered to be more important than any of the others, such as the profit record, etc. It does not seem unreasonable to believe that a regional institution, when considering the application of a company within its region, is likely to be better informed on such a vital matter than one in London. Thirdly, if direct investment by the medium and small investor is to be encouraged, some form of investment leadership such as might be provided by a regional institution would be desirable. Finally, in co-operation with a provincial issuing house and provincial brokers it might enable firms to get an official quotation at an earlier stage in its development and at a lower cost than if it had to depend on the London market alone. Dr. R. F. Henderson has convincingly shown us that in present conditions firms have to turn to the public for new money sooner than before the war and that the costs for a small firm may be prohibitive. I have mentioned above that some of the fixed costs of securing a quotation are much lower in Liverpool than in London (see Table as an indication of the present scope for raising new capital). A possible solution to the problem of costs might be to encourage firms to consider a

Permission to Deal and Official Quotations on the Liverpool Stock Exchange

	1950/1	1951/2	1952/3	To 6th Aug. 1953/4
	£ms	£ms	£ms	£ms
Bonuses	527,750	22,030,500	62,972,000	26,500,000
Rights	4,350,000	28,424,000	5,903,000	1,300,000
Prospectuses	11,900,000	14,250,000	25,633,000	5,840,000
Additional quotations and mergers	253,000	1,626,000	14,936,000	980,000
Quoted already in other S.E. lists and new to Liverpool ..	4,022,000	2,132,000	14,554,000	2,100,000
Reconstructions . . .	Nil	7,200,000	9,692,000	Nil
National placings . . .	1,150,000	Nil	3,886,000	Nil
Local placings . . .	400,000	60,000	125,000	Nil
National introductions . .	90,000	1,000,000	Nil	Nil
Local introductions . .	110,000	1,063,000	480,000	805,000
Total .	22,802,750	77,785,500	138,181,000	37,525,000

provincial quotation as a preliminary step to securing a national one; but an improvement in the Provincial New Issue Markets would be an essential prerequisite.

Whether such a policy is adopted or not, the Provincial Exchanges even more than London must depend for their survival on attracting the small investor, and cannot afford to neglect any possibility of reducing their costs by such devices as centralised book-keeping and accounting. (In fact I think that it is probable that the Liverpool market will introduce such a system within the next year or two.) It is possible also, since a vigorous search for business is essential, that they will have to revise their attitude towards advertising and the use of agency.

In conclusion, the resource and ingenuity displayed in devising flexible policies to combat the present threat will determine whether in a few decades the institution, of which the Liverpool Stock Exchange is my immediate example, will be regarded as a relic of *homo capitalismus*, condemned to extinction as surely as the slave trade of an earlier Liverpool, or be a vigorous market performing a useful economic function.

A TWENTY-YEAR FOLLOW-UP OF INTELLIGENCE TEST SCORES[1]

by

DR. J. D. NISBET

INTELLIGENCE tests have been criticised for almost as long as they have been used. Their continued use in the face of such constant attack rests on their well established predictive value for many types of selection and guidance. This paper is concerned with one further such validation of intelligence test scores—in this case predicting the success after twenty years in the teaching profession of a group of graduate teachers. The results which will be given show that an intelligence test can be a valuable help in the prediction of success in teaching.

I am indebted to Prof. Rex Knight, of the University of Aberdeen, for the intelligence test scores, which he obtained from his post-graduate students during the years 1930–34; and also to Mr. Hardie, Director of Studies of Aberdeen Training College, who preserved and made available the records of teaching assessments which were used in this inquiry. My task was to trace the university records and subsequent careers of the 335 graduate teachers for whom test scores were available; in the end, information was obtained about 266, or 80 per cent. of the total. Of the 266 traced, 28 had changed to professions other than teaching, 14 had emigrated, and 2 had died: the exclusion of these left a group of 222 teachers (92 men and 130 women), on whom all the following calculations are based.

The investigation was planned according to the standard follow-up procedure. Predictive assessments obtained before the group entered employment were compared with a criterion of success after twenty years' work.

It is, of course, difficult to find a wholly satisfactory criterion of success in the teaching profession, for one person's opinion of a successful teacher is often different from another's. The criterion used here was the level of responsibility of the teaching post held in 1953, with responsibility assessed on a five-point scale. It is conceded that some excellent teachers may not achieve promotion, or even may not seek it. However, the criterion of achieved advancement in the profession is accepted as the best available. The five-point scale of responsibility by which promotion was estimated corresponds closely to income on the Teviot Scale which governs the salaries of teachers in Scotland. In case objection might be taken to such a measure of responsibility, all the calculations were re-worked using bi-serial correlation—that is, with a two-fold classification separating those who had achieved promotion from those who had not; and the results by this alternative method were closely parallel to those obtained from the five-point scale of responsibility of the post held.

The four predictive measures which were compared with this criterion of success were an intelligence test, two subjective estimates of teaching ability, and an assessment of undergraduate performance in university studies.

The intelligence test was a 45-minute version of the Simplex Group Test. Since a shortened version of the test was used, the published norms were not applicable and raw scores in the test were used: these ranged from 89 to 33, with a mean of 62 and a standard deviation of 10·6. Norms drawn up by Prof. Knight, based on the scores of 705 Aberdeen University students, show that this group was a representative sample of the university population.

[1] Paper delivered to Section J (Psychology) at Liverpool on Thursday, September 3, 1953.

The first subjective estimate was the teaching mark awarded by the student's supervisor after a year's observation of his teaching practice. This was a percentage mark, and ranged from 85 to 60, with a mean of 69 and a standard deviation of 3·6. The other subjective estimate was a general college assessment of teaching capacity, on a four-point scale, Fair, Promising, Very Promising and Excellent.

Finally, a classification of university performance was obtained from the records of classes attended and type of degree obtained: this was done on a five-point scale, ranging from a first-class Honours degree to an ordinary pass degree with a minimum performance in subjects studied.

These four predictive measures are here referred to as Test (for the intelligence test score), Teaching Mark (for the subjective estimate of teaching ability), Teaching Capacity (for the general college assessment), and University Degree (for the standard of undergraduate performance in the university). The way to find out which of these measures is the best predictor of success in teaching is to compare the results from each measure with the accepted criterion of success. The measure whose results show the highest correlation with the criterion is thus the best predictor of success. The results are as follows.

The correlation between criterion and test—that is, between the level of responsibility of the post held in 1953 and the intelligence test score in the early 1930's—was + 0·26 in the total group of 222 teachers. The correlation between criterion and teaching mark was − 0·05; the correlation between criterion and teaching capacity was − 0·02; and the correlation between criterion and university degree was + 0·36. The first and the last of these were significantly different from zero. That is, the best predictor of success in the teaching profession is university performance; the next best is the intelligence test; and these both give valuable indications of the level of ultimate success, though in neither case is the correlation high—a good degree or a high test score is no guarantee of success in teaching. The teaching mark and the general estimate of teaching capacity appear not to give any indication of subsequent success in the profession.

Included in the total of 222 were 78 married women, and since their professional status was affected by their marriage, the correlations were re-worked excluding them—that is, on 144 cases. There was virtually no difference in the result: the correlation between criterion and test was + 0·24; between criterion and teaching mark, − 0·02; between criterion and teaching capacity, − 0·04; between criterion and university degree, + 0·38. Again, the first and the last were significantly different from zero.

Although these results establish the value of the intelligence test as a predictor of later success, the correlation of + 0·26 between the criterion and the intelligence test is not very high. However, it must be remembered that the subjects were all graduate teachers and had undergone severe selection for intelligence, first in secondary school and then in university; each selection would inevitably reduce any correlation involving intelligence. On the other hand, the correlation coefficients involving university degree are artificially boosted by the system of promotion in teaching: the type of degree which one has is known to those who confer promotion and is a direct factor in determining the promotion itself. Mere possession of a good degree increases the likelihood of promotion, since it is immediately brought to the notice of the appointing committees; whereas the intelligence test scores were not known to anyone, and any relation between test score and success must be due to some common or general factor.

Since university degree is thus a direct factor in promotion, it was decided to hold constant university degree in a partial correlation—that is, to calculate the relation between criterion and predictors among those who possessed the same or equivalent degrees. The results of this partial correlation were as follows. With university degree held constant, the correlation between criterion and test score was + 0·18 in the whole group and + 0·17 excluding the married women. This is again significantly different from zero. The partial correlation between criterion and teaching mark was − 0·09

in the whole group and — 0·05 excluding the married women. The partial correlation between criterion and teaching capacity was — 0·14 in the whole group and — 0·10 excluding the married women. These results suggest that among those student teachers who possess the same or equivalent university degrees, those who do well in an intelligence test are likely to achieve promotion in their teaching careers ; whereas a high teaching mark or estimate seems to give no such indication for the future.

Thus the objective test comes out well in comparison with the subjective estimates of teaching ability. However, this paper is not intended to present a conflict of objective versus subjective methods of assessing ability. The subjective estimates were essentially estimates of *teaching* ability, and the objective intelligence test was a measure of *general* ability. The results may mean only that teaching ability as assessed at the Training College stage is not a factor which makes for advancement in the teaching profession. On the other hand, when we break down the total number of teachers into smaller groups, some interesting results emerge which allow a reconciliation of the objective and subjective methods of assessment.

For example, if the results for men and for women are taken separately, the correlations between criterion and predictors show some differences. In the group of 52 single women, the correlations are much the same as in the total group : between criterion and test, + 0·29 ; between criterion and teaching mark, — 0·12 ; between criterion and teaching capacity, — 0·06 ; between criterion and university degree, + 0·45. In the group of 92 men, the correlations are in a different order : between criterion and test, + 0·06 ; between criterion and teaching mark, + 0·16 ; between criterion and teaching capacity, + 0·21 ; and between criterion and university degree, + 0·33. The smaller numbers, of course, imply less reliability in these correlation coefficients, but it seems that in the smaller group of men alone, the subjective estimates predict better than the objective test.

The major weakness of subjective estimates of ability is that they tend to inaccuracy when numbers are large. Small groups can often be arranged in order of ability quite accurately by persons who know the individuals ; but with large numbers, subjective estimates cannot maintain consistent standards, or allow for differences in average standard which may exist between sub-groups. The objective test has exactly the opposite weakness : it measures accurately the average standard and the spread of ability, but it is liable to go wrong in individual cases. One solution is to combine the good elements of each measure by the process of scaling, used by McClelland in *Selection for Secondary Education* and described in detail by McIntosh in *The Scaling of Teachers' Estimates*. In the scaling process, the intelligence test scores are used to fix the mean and standard derivation of each sub-group, and the order of merit within each sub-group is determined by the subjective estimates. The statistical technique of scaling is easier to carry out than to describe.

Such a scaling method was applied to the data here. The teachers were divided into four sub-groups : ordinary degree men, ordinary degree women, honours degree men and honours degree women. The teaching marks were scaled on the intelligence test scores, the test scores determining the relative positions *between* groups, and the teaching marks determining the positions *within* each group. This combination of objective and subjective measures gave a higher correlation with the criterion than either measure separately : the correlation between criterion and scaled mark was + 0·33 in the total group of 222, and also + 0·33 in the group of 144 when married women were excluded.

The value of the intelligence test can thus be demonstrated by statistical analysis. Those who find statistics unconvincing may prefer a straightforward way of showing how an intelligence test may give accurate predictions even in a highly selected group of university graduates. We can pick out individually those who scored very highly in the intelligence test, and contrast their careers with a group at the other end of the scale, who scored very low in the test. There were 18 high

scorers (who had scored 80 or over in the intelligence test) and 15 low scorers (who had failed to reach a score of 45 in the test).

The university and professional careers of the 18 high scorers have been generally successful and even distinguished. At university, 12 of the 18 high scorers took their M.A. degree with honours, 4 first class, 6 second and 2 third ; 2 took the double M.A., B.Sc. degree ; and the other 4 took good ordinary degrees. None got only the bare minimum pass degree. In their later careers, 5 left the teaching profession, 1 man to be a clerygman and 4 women to be married ; 2 were not traced ; of the other 11, 8 are now in posts of responsibility, and 3 have not gained promotion.

The picture presented by the careers of the 15 low scorers is a very different one. All took ordinary degrees except one who took second-class honours and one third class. Eight of them obtained their degree on the bare minimum of performance. The subsequent careers of this group were difficult to follow : 6 could not be traced ; of the rest only 4 are still teaching, and only 1 of these is in a post of responsibility, in a small school.

The contrast between the careers of the high scorers and the low scorers is striking, though of course there are the inevitable exceptions which prove that the intelligence test does not predict with certainty. However, the evidence given is surely enough to show that, even in a highly selected group of university students, an intelligence test lasting only 45 minutes can be a valuable help in indicating the probable trend of a career, whether for better or for worse.

SCIENCE AND THE UNPREDICTABLE

A DISCUSSION on Science and the Unpredictable took place at Liverpool on Sunday, September 6, 1953, under the auspices of the Division for Social and International Relations of Science. Contributions were made by Prof. L. Rosenhead, F.R.S., and Prof. T. H. Davey, of Liverpool University, and by Dr. G. L. Kesteven and Dr. K. V. L. Kesteven, of the Food and Agriculture Organisation of the United Nations, Rome. The papers are introduced by Mr. Ritchie Calder, C.B.E., Chairman of the Division.

INTRODUCTION

by

Ritchie Calder, C.B.E.

Two popular misconceptions, which are detrimental to science, are the sense of 'scientific predestination' and the illusion of scientific infallibility. People tend to believe, with a fatalism which is unhealthy for science, that there is an inevitability—Einstein says '$E = mc^2$' and Hiroshima and Nagasaki are eventually destroyed. And, at the same time, they tend to think of scientific research as a coin-in-the-slot machine—somebody puts in the penny and science can provide the answer, any answer to any problem.

The Division for Social and International Relations being concerned, within the British Association, with the social implications of science, took the occasion of the Liverpool Meeting, to discuss 'Science and the Unpredictable.' The object was not to widen the Principle of Uncertainty but to show that scientists proceed within the limitations of their knowledge, that the *Unpredictable* is what lies outside that knowledge, since scientists can predict only on the basis of established facts.

As Professor Rosenhead points out, the methods of prediction in the exact sciences are not necessarily applicable in the other sciences. And, as the other speakers show, there are 'unpredictable' factors in the medical and biological sciences, some potentially dangerous—like the development of resistant-germs—and some potentially beneficial—like new discoveries in marine biology or in agriculture which can, or may, materially affect the well-being of mankind.

PHYSICAL SCIENCES

by

Prof. L. Rosenhead, F.R.S.

THE word 'Science,' in its broadest sense, is synonymous with learning and knowledge, but in the Physical Sciences, the word assumes the rather specialised meaning of 'systematic knowledge acquired in a special way.' The Physical Sciences have developed a special technique for finding the answers to their problems; the essential feature of this method is that it must enable one to predict with accuracy the results of experiments not yet carried out. It has proved to be extremely successful and its success is measured by the accuracy with which it enables predictions to be made.

The recent spectacular achievements of Applied Science, especially the creation of new and terrifying weapons of war, have caused many people to cower in fear and shake their fists at the bogey—Science. Science, they say, is leading mankind to disaster. That is nonsense—complete, unadulterated nonsense! In their irrational fear people forget the many astounding benefits conferred on mankind by the application of the scientific method. It would be more proper for us to count our scientific blessings, and realise that the same method carries within itself potentialities for great good, or great evil, or both.

To the physical-scientist the word 'unpredictable' is a challenge, and there are

very few who do not respond to it. If people find that they are unable to predict with accuracy, the reason may be one of several. Firstly, it may easily be that they are just not clever enough. If this is so there is nothing for them to do but to work hard, think clearly, and hope that one day wisdom will come. Secondly, people may *think* that they are clever enough, but their ideas may be completely wrong. This state of affairs can often be brought to light in the physical sciences by simple reproducible laboratory experiments, or by tests of logic. Sometimes, however, the system of logic may be correct but the logician may be in error, so that a well-designed experimental check always seems to be desirable! Thirdly, even though the ideas may be correct, or partly correct, people may still be unable to predict accurately, simply because they may not know all the facts. For example, we used to hear a great deal about the unpredictability of the English weather, and also about the unpredictability of the behaviour of charming young ladies. However, as an ardent devotee of the B.B.C. weather forecasts, I have come to the conclusion that the forecasts are remarkably accurate. There has, in fact, been a great improvement in forecasting in recent years. Why? Not because the fundamental methods of weather forecasting have been altered, but because the appropriate authorities now have access to many more observations in various parts of the world, of wind-speed, wind-direction, gustiness, temperature and humidity. The more information which can be put into the scientific machine, the greater the reliability of the final answer. In much the same way, I have been told—by those who have studied the subject with care—that the unpredictability of the behaviour of charming young ladies is only apparent in the absence of detailed knowledge; the more one knows about them, the more accurately can their behaviour be predicted.

The important thing is that theories are speculations based on known facts and are as strong as the facts on which they are built. It follows that we must be prepared, and willing, to alter our speculations in the light of new facts whenever these become available. All true scientists must hold their theories lightly, for one well-tested simple fact may be strong enough to upset a vast edifice of theory. It is the story of David and Goliath. The laws of Science are simply working rules which apply, with varying degrees of accuracy, within the limits of existing knowledge. If we step outside these limits, then it is often legitimate to question the validity of those rules. Physical scientists often make wrong guesses when they venture into the unknown—and they frequently make mistakes within the bounds of existing knowledge, where they ought to know better! They make mistakes like anybody else, but they have a system which automatically checks statements and guesses and so reveals error.

There is the further possibility that some day we may really find a branch of human thought or activity in which the Unpredictable does hold sway. If this should ever occur we may be close to the core of the metaphysical problems of existence—but we shall be beyond the realms of physics. But who is there in our generation wise enough to recognise the Unpredictable when he sees it? If some should maintain that *they* can isolate such regions of human knowledge, the scientist can only reply with humility, 'Perhaps you are right—but I would like to investigate the accuracy of your statements.'

The simple picture which I have just tried to sketch was accepted, albeit grudgingly, by most lay philosophers until 1926. In that year Professor Heisenberg made known the outlines of a principle which he had then formulated, one to which he attached the name 'The principle of indeterminacy.' The intriguing nature of the title opened up a whole new epoch in lay philosophy, and its exponents said that at last science had indicated its fallibility. There was still room, so it was said, for human free-will, for miracles, and for the intervention of the hand of God. It is such a pity that this rebirth of popular philosophy had to be based on a misunderstanding! Ignorance clouded the picture, and debased the meaning of the word 'science.' What was intended to be a special contribution within a restricted field of theoretical physics was twisted out of its true meaning by people who did not trouble to investigate before they spoke.

No wonder misunderstandings arose and still arise !

Let us consider the way the physical sciences work. The physicist is interested in the properties of matter and energy ; the chemist in the composition of substances and their effects upon one another ; the mathematician in the ideas of number and space ; and the engineer makes use of all their results in the service of society.

The fundamental technique is that of the experimental physicist : accuracy of observation is *his* goal. Next comes the experimental philosopher who looks down upon the battle-field of science and tries to take a bird's-eye view of all the observations and measurements so far made. From his thoughts he evolves ideas which, to him, appear to connect all the known facts and observations. His ideas, his guesses, his speculations, form what we call a theory. Then he tests his theory by jumping into the unknown and predicting what will happen in some untried experiment. The test of his theory is the accuracy of his prediction.

Accuracy of prediction involves precision, and where precision is required, number and mathematics *must* enter. At this stage pure mathematics, or applied mathematics, or statistics, or theoretical physics, or theoretical chemistry, or analytical engineering, make their contributions to science.

The use of statistics is often confusing to a layman, and frequently to professional scientists too ! But the basic idea is simple. Frequently we find that what is happening depends on so many things, some of which we may not understand, that we admit our ignorance and say that we will not attempt to predict individual results, but that we will attempt to predict average values. Consider, for example, the weight of any individual member of this audience. It is virtually impossible to predict what his weight will be in ten years' time for so many factors are likely to enter. But it is found that if we take large numbers of people, weight them, and take average weights within appropriate age groups, then the average weights seem to follow some kind of plan. It may therefore be possible to calculate what will be the average weight of this audience, as a whole, in ten years' time, even though that of any individual may baffle us. On this point, and similar ones, there is a cleavage of opinion among scientists. Some feel that as our knowledge of individuals grows we may be able to improve our theories to such an extent that, in time, predictions of individual characteristics will be made with accuracy. Other scientists say that we shall never be able to do so. At the present time the issue is an open one : the question bears on the two meanings of the word ' unpredictable '; one of these implies that we shall *never* be able to predict accurately, and the other that we cannot predict accurately on the basis of existing knowledge.

There is, however, the further point that in some cases we do not, for practical purposes, *need* to know the complicated life-history of every individual member of a group. When a swarm of locusts is moving we are not usually interested in knowing the path of every individual locust, but we may be very interested in knowing the path of the swarm as a whole. In such problems the employment of the simplifying methods of statics is very desirable.

Science, however, does not rest when a prediction has been made : all scientific prophecies must be tested by accurate experimentation. If the results of theory agree with those of experiment—good ! But even such agreement does not actually *prove* that the theory is correct. All that it does is to give one greater confidence in the theory which has been formulated ; other theories, may, however, be formulated later to provide better, stronger links to connect existing knowledge with facts still to be discovered. The scientific method is thus slow, painstaking and humble.

When theory and experiment do not agree, then comes the critical period. Either the theory, or the experiment, or both, may be in error. The experiment must be carefully checked and the theory must be carefully scrutinised. This process of testing has proved to be extremely fruitful. For example, refinements in methods of measurement led Ramsay to discover Argon, and by so doing to open up an entirely new field of Chemistry. Apart from revealing gross errors, the checking of theory has also revealed more subtle ones. Such discoveries have led to the formulation of new theories, and that,

for example, is how the theory of Relativity came into being. In fact, the discovery of the apparently inexplicable fact has been much more fruitful for science than the discovery of results which fall within the territory of accepted knowledge. In this way, for example, Penicillin was discovered. In the Physical Sciences one is never sure whether some apparently trifling inconsistency, some small abnormality, some puzzling anomaly, may or may not be the key to an unknown treasure-house of new ideas, new facts, new theories, and new discoveries. The unexpected provides the challenge and the pleasure of science !

Even there the matter does not rest. When theory and experiment are once more in harmony, either by alteration of the theory, or by refinement of the experimental technique, the cycle of prediction, testing and harmonising is repeated. And so on without end !

Many people have been confused, however, by the assumption that if a branch of human thought is labelled a ' science ' by its disciples, it follows automatically that accuracy of prediction will of necessity follow. Scientists do not claim this, but the popular mind assumes it. The political, economic and biological sciences provide examples of what I mean. And this confusion has led, in its turn, to the idea that events in Politics, Economics, Medicine, etc., follow simple, inevitable laws, as they appear to do in the Physical Sciences, and that all that remains for us to do is to discover them and to accept their inevitability. I venture to suggest that more harm has come to us from this analogy than from almost any other one. The non-physical sciences to which I have just made reference do not in all their investigations employ the same ruthless, self-critical, controlled methods of the Physical Sciences. Very often, the nature of their investigations does not permit the use of these methods. Controlled experiments on living matter, for example, or in the field of economics, are rarely possible. Strong arguments can, in fact, be advanced that the methods of the Physical Sciences may not be applicable to all branches of the non-physical sciences. It follows that the so-called laws of Economics must be interpreted with much greater flexibility than the laws of the Physical Sciences. So too in certain aspects of Psychology and Medicine ! In the future, perhaps, with an increased understanding, the position may be altered. But at the present time there is not the slightest justification for accepting as immutable all the tentative speculative statistical generalisations which are advanced under the dignified title of Scientific Law or Rule.

It would be improper to conclude this condensed statement, however, without referring to the fact that many important advances have been made in the last half-century in some of the non-Physical Sciences to which I have referred—although to the best of my knowledge they have all come about through the applications of the technique of the Physical Sciences.

To sum up, the lesson of the Physical Sciences is that they form an interlocking, self-checking system, in which theory and experiment are inextricably interwoven. Sooner or later errors in thought or experiment must reveal themselves, provided one adheres to the austere rigour of the game. The acid test of the validity of any theory is its ability to lead to accurate predictions. It must not be forgotten, in conclusion, that one of the most fruitful branches of activity of the Physical Sciences is to investigate reported examples of the so-called Unpredictable, and to find out whether they do not really belong to the domain of the Predictable.

Medical Sciences

by

Prof. T. H. Davey

I must confess to finding some difficulty in dealing with ' Science and the Unpredictable ' because I do not fully understand what it means. It appears to me to involve to some extent a contradiction in terms, because a scientific hypothesis—the basis of an experiment—is an intelligent prediction based on knowledge and past experience.

The scientist always has a plan of investigation and he usually knows within limits what results he is likely to get ; he rarely, if ever, attempts an experiment the issue of which has not been foreseen.

It is true that an unpredicted result sometimes occurs but since an experiment is usually on a small scale the damage is rarely important. The unexpected, however, does happen when the fact established by scientific experiment under controlled conditions is applied on a large scale and not under controlled conditions. The scientist is rarely responsible for these large-scale applications of scientific findings and, if he were, his training would ensure that he proceeded cautiously by pilot schemes while he estimated the influence of factors not under his control. In my view then, the Unpredictable is usually not directly associated with science but rather with the thoughtless and too rapid employment of techniques developed by scientific experiment under conditions in which they have not been tested, in fact the unscientific use of the findings of scientists.

It must be acknowledged that society will at once seize on and employ any scientific findings which appear to confer an immediate advantage. The scientist may wish slowly to work out the relationship of his findings to other environmental factors, but the public will not be content to wait, and some commercial organisations, seeing a market, will be anxious to exploit the public interest. It is, therefore, important for us to consider the question how far does the public know what it wants. It is probably true to say that society grasps at anything which appears to satisfy an immediate want, but it rarely faces its long-term needs, and the two are frequently in conflict. In general, man is more interested in the ' now ' than the ' later,' and indeed will often sacrifice the good of posterity for an immediate gain. Raymond Pearl has said :

' Man's whole history is a record of a struggle, partly by trial and error and partly by the exercise of wisdom, to find easier and pleasanter ways of getting a living.

' The adaptive process by which this end is achieved requires careful watching for the chief characteristic of biological adaptation is opportunism rather than wisdom. In those adaptive efforts the tendency is to take the easiest way and all too often the easiest way has led to the extinction of the species in the end. The adaptations that may be regarded as examples of wisdom may perhaps only be naturally selected instances where opportunism has happened by pure chance to coincide with the wisest course in the long run. Those cases where this perfect coincidence failed to be present are not now seen for they no longer exist to be observed.

' The advancement of knowledge and science generally represents biologically the most immense and intricate adaptive procedure ever achieved by any organism —but has it brought collective wisdom in its train ? '

Sentiment, too, influences decisions on policy and may cause serious long-term problems. Nature used to deal ruthlessly with those who were unfitted to survive, but we have decided to reject the law of tooth and claw, and I am not sure that we have not substituted a maudlin sentimentality for common sense. Medical science has in some fields reached such a high technical level that it can enable individuals physically to survive who before would have perished, but it often cannot provide the mentality or personality which allows them to live in the full sense of the word.

A third factor which is responsible for unexpected and grave results from the application of scientific findings is the speed with which they are applied. Man lives in ecological balance with his surroundings and any alteration of that balance produces a reaction, often a chain reaction which grows in magnitude as more and more factors become involved. So long as the reaction is wisely directed and controlled the ultimate benefit is out of proportion to the initial stimulus, but it is equally true that grave detriment may result if the reaction is not carefully controlled. Rapid alteration of the ecological balance produces instability, and the conditions in which stability is subsequently re-established may be seriously to man's disadvantage. Any policy which affects man's ecology must, therefore, be very carefully considered and its implementation should be slow in order that the resulting reactions may be watched and controlled, and if necessary other steps taken to counteract instability of the balance. Nature never moves in a hurry and man risks catastrophe if he attempts to hurry natural processes.

There is a tendency on the part of some people to attack science and scientists because the final result of the application of the advances of science has been serious, even disastrous. For example, I have seen it suggested that scientists should be controlled because their work has produced such things as the atom bomb. This is quite unjust to the scientist and is an irrational attitude because it was public pressure in the form of government orders which directed the scientist into producing this terribly destructive weapon. It would be unwise to restrict research on the ground that subsequent developments may lead to unpredictable results. Most of the techniques discovered can be used to man's advantage even though they may also be employed for his destruction. What *should* be restricted is the use of new developments to the detriment rather than to the benefit of man. This calls for control, not of the scientist, but of man in the mass, for it is public pressure which demands the use of the new techniques in wrong connections and before they have been fully investigated.

Let us briefly examine some of the results of the use of recent scientific advances, which are causing concern at the present time. The early good results obtained following the administration of new chemotherapeutic substances, the sulphonamides and the antibiotics, are no longer being obtained because in some cases the organisms which they were employed to control have become resistant to their action and can survive and multiply in spite of them. The resistance resulted from two things, improper dosage and the survival of the more resistant strains by a process of natural selection. Under-dosage increased the naturally existing power of resistance when it was present in the organism, and the death of the least resistant strains allowed the more resistant to be propagated and communicated. As a result of these two processes African sleeping sickness has in the region of Leopoldville become so resistant to tryparsamide that about fifty per cent. of the cases encountered there cannot be successfully treated by the drug. This is a serious matter because there is at present no satisfactory alternative way of treating the later stage of the disease, and it has become necessary to organise an entirely different method of attack. This involves periodic injection of the whole population, at risk, with a drug which will protect them for a period of months.

A similar result has been obtained in this and other countries where streptococci, the tubercle bacillus and other organisms are exhibiting resistance to the antibiotics and other chemotherapeutic agents which it was hoped would control them. These preparations were issued for general use before full details had been worked out as to dosage and the type of case in which their use was likely to be most valuable. This, coupled with the insistence of patients that they should receive these new and powerful drugs, has resulted in their use in minor infections which would have recovered without such treatment, and in the development of resistance in the organisms. This situation can be combated in several ways ; valuable drugs should be kept for cases in which they are necessary and in which other lines of treatment have failed ; new drugs are being sought all the time which will be effective in cases where resistance to existing drugs is encountered. In addition, before new chemotherapeutic products are issued for general use the attempt is made to define the conditions of satisfactory use and the risk and degree of drug resistance.

The host-parasite relationship is another matter which we might consider for a moment. Man, in the confidence created by his great advances in chemistry, has adopted the attitude that disease must invariably be attacked. Nearly all infecting organisms stimulate resistance on the part of the infected individual and in many diseases the patient recovers without ill effect and with the benefit of having acquired an immunity to re-infection which persists for a short or long period according to the nature of the illness. To cure the infection before immunity results leaves the patient defenceless against future attacks. It would seem to be wiser policy to watch the progress of the infection and only to intervene and bring it to an end if the patient is failing to master it. Early interference, and the attempt to prevent infections which are normally mild, results in a postponement

often from childhood to an age when infection is economically undesirable or even much more serious ; mumps, for example, in an adult can cause serious sequelae while in childhood, though very uncomfortable it is usually an infection of no consequence.

In those infections which normally result in serious damage or death active and immediate intervention is necessary. Where possible, however, a method of immuno-prophylaxis should be developed which will create an innocuous infection that gives immunity to subsequent infection. Such a method has largely solved the problems in diseases like smallpox, diphtheria and yellow fever.

The tendency to over-population of the world, which exists mainly in the tropics, is often regarded as an unpredictable result of medical advances. Two comments must be made about this view. Firstly, the present situation was predicted by Malthus in the eighteenth century. Secondly the population increase which produced the existing demographic position resulted, not from medical activities, but from the establishment of stable governments.

Steady increases in population have been noted in tropical countries over the past seventy years. In those countries especially in which modern public health techniques have been introduced in the last decade, rates of population increase have been astonishing. This has resulted from a dramatic and rapid reduction of the death rate accompanied by the continuing high level birth rate characteristic of under-developed communities.

In Ceylon, for example, where more people died from malaria than from any other cause, the use of modern insecticidal methods of control has reduced the death rate by almost 50 per cent., to below 12/1,000 while the birth rate remains unaltered at some 40/1,000. Since Ceylon only produces enough food to feed about half its population, the increased rate of population growth from 80,000 annually before the recent malaria control programme to some 200,000 subsequently has very important health and political implications. Various national and international organisations have provided Ceylon with large funds for the purpose of clearing jungle for agriculture and for irrigation schemes, but these projects take time to produce results and the number of farmers settled on new land is only 10,000 a year, a number which, with their dependants, represents about 50,000 new settlers annually, and only a quarter of the annual population increase. Unless food production can be so increased that each farmer produces enough to feed six or seven people in addition to himself and his family, standards of nutrition will fall below the already inadequate level and political unrest, which is now in evidence, will increase and may well cause chaos.

Ceylon is not an isolated example of the rapid and adverse effect of modern public health techniques on local demographic situations. The same type of result has been seen in Cyprus, British Guiana, Mauritius and in districts of other countries. The degree of population pressure which has ensued has varied according to the pre-existing population density and the level of technological development of the territory. It must be clearly understood that the term *population pressure* is without meaning except in relationship to the state of development of the country's natural resources. England is at present suffering from population pressure which has revealed itself in the form of rationing but, even with our dense population, there would be no pressure if, for example, we produced considerably more food or coal and could freely sell our manufactured products abroad. Pressure results from failure to employ to a sufficient degree the natural resources and technical skill of the territory and its people. In this country medical measures are unlikely to aggravate seriously or rapidly our demographic problem but the situation is different in technologically under-developed countries with a high birth rate. In such countries mortality is mainly due to parasitic diseases which are readily controllable by modern methods. The World Food and Agricultural Organisation is trying to improve the feeding of people in these territories but their techniques necessitate changes in the practices of subsistence farmers and will inevitably progress relatively slowly. Modern methods of control of the major

tropical diseases, however, can produce vast changes in mortality in two or three years and can, therefore, create a problem of population pressure which will more than counterbalance the increased food production resulting from F.A.O.'s efforts. Such pressure is, in fact, already evident.

The problem is not, however, insurmountable and I would like to indicate one possible solution. Capital is limited in under-developed areas and such capital as is available must be devoted to raising standards of living, in the first instance by increasing food production. Where agricultural productivity is limited by disease, public health activities must have priority but only if their effect is to raise output and standards of living. Irrigation schemes and other forms of capital investment in agriculture should be a first charge on whatever funds are available. Utilisation of natural resources such as minerals, economic crops and so on, should follow and possibly industrialisation also. Urbanisation which will necessarily result from industrialisation, has been associated in the western world with a decrease in the birth rate. To some extent this was probably the result of education and of economic pressure following a demand for the amenities associated with a higher standard of living ; it also resulted from the establishment of a wage-economy which tends to postpone the average age of marriage. All the factors tending to increase productivity and to reduce the birth rate should be encouraged in the tropics, and all development plans must be integrated and directed towards the raising of standards of living. Medical advances must be timed and related to the general and economic progress of the country if benefit is to accrue to the people. Failure to integrate medical policy into the general plan of development may be responsible for a rate of population increase which will counteract the increase of productivity.

This is a very brief statement of the way in which I believe the benefits of western technology may be introduced into under-developed territories without causing a demographic problem. The essential thing is to maintain an ecological balance and to proceed at a sufficiently slow pace to permit modifications of the plan when one line of advance is seen to be creating a problem which may threaten the whole project.

Marine Sciences

by

Dr. G. L. Kesteven

Since the ability to predict involves both knowledge of the system whose behaviour is to be predicted, and a competent awareness of that system, the so-called Unpredictable in science may not be dismissed simply by a parlour study of the ' happy accidents ' which have marked the progress of scientific discovery, or, conversely, of the tedious treks by which that progress has been achieved. Obviously the expression signifies both the unpredictable in the history of science and the unpredictable in the things which science investigates.

The word ' unpredictable ' carries for me two meanings : the unknowable and the unexpected. There are some things which occur when, as it were, we are not looking ; when we are off our guard, or unprepared ; or which occur at times or places other than those at which we expected them to occur. These are the unexpected, the surprises, the happy accidents ; they are also the unhappy errors, the mistaken identities whose pursuit consumes precious hours—even valuable lives. Then there are the phenomena which are so obscure that the intricate tangle of their structure is impenetrable by the spotlight of our enquiry ; and, by contrast, there are others which are apparently glass-clear but whose component parts are so busy, so flibbertigibbet and erratic that no one would care to gamble on what they might do in the next two minutes. And these, the obscure and the erratic, are the unknowable so far as any present ambition to predict is concerned.

This distinction offers two avenues of approach to the question of what is unpredictable in science, and we may hope that these two avenues will come together again at some commanding position from which we may form some opinion on whether or not science will go forward at steady pace along some fixed and predetermined, predictable path to the ultimate revelation of all mystery.

It is unnecessary to attempt now an analysis of the logical basis of science in an effort—which I am sure would be vain—to measure the speed with which science advances, or to lay before you some navigational laws by which science is steered. I have assumed that one purpose of this discussion is to examine some of the surprises that have happened in the past in order, if possible, to discover firstly, why they were surprises ; and secondly, their nature, and the effect of their surprise element. We may then speculate in general terms on the kind of surprise which might perhaps befall us in the future; which is, of course, paradoxical.

The quality of the surprise which we get depends upon our attitude at the time : we may be looking in the wrong direction, although perhaps we may be looking intently and expecting something ; or we may be looking directly and correctly, and yet, for various reasons, failing to recognise what we see. Or it may simply be that, pursuing a devious and tortuous path through, shall I say, a deep boscage of experimental and field experience, past great view-obscuring accumulations of data, we suddenly arrive at a vantage point—an all-revealing explanation, an all-describing law which is totally unexpected.

At this point I must make an important reservation in distinguishing between the surprises which have occurred in the course of scientific enquiry, and the amazement and delight which overtake us, individually, when we meet something which is new to us. The events or objects which evoke such emotions are not surprises in the sense of which I speak, and this distinction is of especial importance to me in dealing with the sea and its mysteries. For this is another world, peopled by creatures of extraordinary beauty and incredible design. To fishery workers this world is something of a liability because there is joined in the mind of the average person, at the one time, the ideas of 'stinking fish' and the mystery of the sea, so conflicting as to occasion ribaldry. And yet, for all the fun, who has measured the immensity of the seas? I do not refer to their area, nor even to their depths, although surely we have much more to learn about the depths and the configuration of the bottom of the oceans. But, as examples, I speak of *Latimeria*, of the amazing growth of the South African pilchard fishery, and of the development of the new shrimp fisheries in the Mexican Gulf. *Latimeria* was a surprise, not in itself, nor, for instance, in its anatomy, but in its survival. It was a surprise because of a major aspect of fishery work, namely, that in general our objects of investigation are concealed from us below the surface of the sea. The bathyspheres of Dr. Beeby and Professor Picard have carried men down to great depths and given them glimpses of unsuspected worlds and their creatures. Cousteau's aqualung has conferred upon man a movement virtually in another dimension, and although his explorations have so far been within fairly well-known communities, the prolongation of the observational time opens the way to a more intimate acquaintance with the life (rather than the form) of these creatures. But even further, the under-water camera, underwater television, acoustic apparatus and other equipment are projecting our nerve receptors deeper and more persistently into this other world. Thus, whilst we cannot say that there will be or will not be further discoveries such as of *Latimeria*, we can say that we believe that there are many more new creatures to discover in the sea, and that our means of reaching them are steadily being improved.

For the fishery industries these strange creatures are of less importance, at any rate for the time being, than are certain species, relatively few in number, which occur as populations of great abundance. To untrained observers any scattering of fish in an area of the sea is to be taken as evidence that the area teems with fish and is immensely rich, yet so often such descriptions are wildly astray from the truth. To locate large resources, hitherto unknown or merely suspected, and to secure the development upon them of substantial fishing operations, is no easy matter—nor can the progress of such a venture be predicted with any precision.

One could name various industries whose development in recent years has proved to be something of a surprise, although, it must be noted, the presence of

the resources was predicted on reasonable grounds. I refer to certain new shrimp fisheries of the Americas, and to the South African pilchard fishery. The latter is especially interesting ; it has been developed deliberately with government assistance on the basis of evidence of the existence of pilchard stocks in these waters. The discovery of the presence of these stocks was no greater surprise than any other discovery of a new species or addition to our knowledge of the distribution of a known species, but I question whether the scientists who first recognised the commercial possibilities of these stocks could have thought that their abundance would prove to be as great as it has.

Among the discoveries which contained some element of surprise we may include the work of Johan Schmitt in revealing the life-history of the eel. This is not the place to describe this life history in detail, but one must emphasise that the elucidation of this mystery took many years of arduous work, and the final satisfactory establishment of the principal features of this life-history was a surprise perhaps even to Schmitt himself. It seems to me incredible that from the scraps of evidence available to Schmitt when he began his investigations, anyone could have foreseen the tediousness of the journey which he would be obliged to make, or could have even guessed that the answer would be that the eels which occupy the head waters of the rivers make an immense migration across the ocean to spawn in one special area and then that the young eels migrate back across the ocean to take their parents' place in the rivers.

Of similar kind is the elucidation of the life-history of certain Penaeid prawns : a story pieced together by workers in the Old World and the New. This enquiry involved the examination of thousands of prawns of almost every size, and, in the smaller sizes, of numerous kinds ; it was a jig-saw puzzle in which the pieces of many puzzles were thrown together ; in the end it proved that the maturing prawns went to sea, like the eel, to spawn, and the young prawn, after various metamorphoses, returned to the coastal lakes from which the adults came, and then fed and grew until in time it became their turn to migrate to sea.

In both these stories the critical point was probably the identification as young eel, or as young prawn, of organisms which little resembled the parent organism. But in the case of the prawn the pursuit was made easier by the knowledge which was then available that crustacea did undergo such transformations in the course of early life. But during the investigation the research workers had the young stages of many species of crustacea to separate from the developmental series of the species under investigation. These stories differ in quantity rather than in kind from many other stories which tell of patient enquiry eventually establishing the connection between apparently unconnected events. I suppose that the conclusion must be that the measure of surprise in any of these discoveries is determined by the degree of our ignorance at the beginning of the search. This, of course, brings me at once into the field of the unknowable.

In fisheries work we are concerned with many enigmas, some of which concern the basic productivity of important areas of the seas ; others concern the life-history of various species of fish ; still others the abundance of different species and the intricate mechanisms which control the population levels of these stocks, and this latter enigma is further complicated by the behaviour of the fish, which determines the entry of fish to the fishing-net.

We know relatively little yet about the physiology of fish ; either of nutrition or of behaviour. And whilst we know a great deal of the life-history of a few species, there are other species about which we do not know much, and there are areas of the seas about which we know next to nothing —barely even the kinds of fish which inhabit them. There seems to be no reason why any of these enigmas should remain unsolved, although it is obvious that we cannot forecast when the solution will be found.

Thus, for example, although measurement of basic productivity of the tropical seas is a major task, there is no reason to suppose that we shall fail in it. From time to time workers have offered the opinion that these seas are less productive than those of the temperate regions, and they have offered in explanation certain observations on temperature, light values, water

transparency, plankton abundance, and so forth. There are yet other workers in this field who are not satisfied, and I hesitate to speculate on what will follow the settling of this controversy. The basic productivity of the sea is, from the fisheries point of view, fundamental to the entire commercial value of these waters, and the validity of our understanding of this basic property affects the usefulness of our plans for exploitation. There are many areas of the sea where we know that valuable kinds of fish are present, but we do not know in what abundance they occur. By contrast, there are other areas in which fish of particular species occur at times in great abundance (or did occur) but which at other times virtually disappear. Such an instance is the California sardine ; another is the fishery of the Indian west coast for oil sardine and mackerel. We have here the task of measuring fluctuations in total abundance, and changes in the distribution of a species.

The foregoing enigmas present difficult questions which we have been able to formulate on the basis of what we now believe we need to know in order to achieve a sufficient mastery of our marine resources and thereby to be able to cope with our food problems. In a different category and, as it were, with a double jacket of mystery, are various phenomena of the seas which have been observed but which have not been understood and whose possible significance in relation to our more important problems is not yet revealed to us. It would be difficult to give useful examples of this type of enigma, which may be said to resemble (say) the unnoticed rhythmic echo of a car as it passes the pylons of a bridge and which for many years past has conveyed the essence of radar, and yet was unremarked in this sense. Perhaps the colour of some marine worm, perhaps the spawning habit of some fish, will one day be seen, after having been before our eyes for centuries, to have been trying to give us some extremely important message.

What I have just called enigmas are phenomena or events whose structure and processes are at present unknown to us, and whose behaviour, consequently, there is no prospect of being able to predict. In contrast, there are systems whose structure is known to us but within which we have, as yet, been unable to measure the relations between the component parts. This may be either because these relations remain obscure or because the behaviour of certain ones among the others is too erratic. As an example I may cite the problem of the connection between the numbers of parent breeding individuals and the number of full-grown stock they produce. In a stock of fish there are at each spawning period numerous individuals to produce the eggs from which will spring the next generation ; we would like, among other things, to be able to predict from the number of spawners the number of individuals from among those eggs which will survive to become recruits into the fishable stock. Sometimes we can make a reasonable estimate of the number of spawning individuals and also of the number of eggs which they produce. Sometimes we have a measure of the intensity of the mortality to which the eggs are subject, and we have some idea of the factors which cause mortality of the eggs and subsequently of the larvae, the post-larval stages and the young fish. In no case, however, are all the factors which contribute to the mortality or survival of the group precisely listed and measured, and thus we are yet unable to predict from the breeding stock of one year and from other evidence the order of magnitude of the brood which will enter the fishable stocks in, say, two years or more. To some people this task appears too great and the advantage which it would give us over the simple advantage of measuring a brood strength at the time that it begins to enter the fishable stocks would be too small to warrant the expenditures which would be required in order to build up and to analyse the vast array of data which would be required.

Yet, in fisheries work, as in all other fields, the tract of the unknown and unmeasurable, and therefore unpredictable, is steadily diminishing and in the newly exposed tract there has appeared a concept which is itself surprising, namely, that we shall not always need to be merely hunters of fish but may in time become fish-husbandmen. In commercial fisheries the great problem is the element of risk : the risk of no catch, of lost catch, or

spoiled, or badly marketed, or cheaply sold catch. A cumulation of risk which has its primary cause in the uncertainty of the fishing operations themselves. If at some time in the future the accumulation of knowledge of the fish stocks and their environment permits the evolution of a bio-technology, which we shall call fish-husbandry, our fishing operations will be conducted with the same sort of deliberate plan as that by which the cattlemen of Argentina and Australia draft and cull and send their stock to market ; taking fish in right quantities of right size with efficient operations by fleets of size proper to their task. It will not be easy ; nor will it come quickly ; probably there will always be some resistance to such management, and there will always remain a risk element. But this is something on which I, for my part, am willing to risk the prediction that it will come true.

There is something further to add here, namely, that since we now can see this goal we must take certain steps toward its realisation as early as is compatible with technical, economic and political considerations. I would suggest that for the scientific world this means, in part, that fisheries science must be recognised as a distinct field of effort which is analogous in many ways to agricultural and veterinary science. When I see that the British Association has a special section for agricultural science, I wonder whether within the near future there might not also be a section for fishery science : for whilst we enjoy the company of the zoologists, we also need the help of botanists, chemists, physicists, economists and others, and our work has a special character of its own.

To summarise my conclusions concerning science in general :—

One : its history will always be marked by discoveries which will be surprises, and its pace and direction cannot be fixed nor predicted.

Two : presumably all phenomena of the physical world will some day reveal to us their secrets.

Three : some systems, however, will long remain unpredictable, and others will remain unmeasured, being trivial.

But, if I do so summarise, I would also add the formulation of a belief that science will take us where we wish to go—perhaps too slowly sometimes ; perhaps sometimes too quickly—but since it is given to us to know something of what we want, and since we are not always wayward children, we may within limits choose the direction and pace of science. And, finally, I would say that to speak of the unpredictable in science is in a way paradoxical because the central function of science is to predict. The most characteristic feature of a scientific experiment is that it aims to display things in such a way that the experimenter may say : if such and such is done under such and such conditions, this and that result will follow. And in natural systems to say : if this and that happen, then also this other will appear. In fishery science the function of prediction is of great importance, for in this field, working as we do with so much of our material concealed from us, our task is to devise ways of being able to tell fishermen where to find fish, when to find them, the abundance in which they will appear, the fluctuations which will take place in that abundance, and the effect which their fishing operations will have on the stocks of fish.

Agricultural Sciences

by

Dr. K. V. L. Kesteven

In the field of agriculture, many people, like the Malthusians and the anti-Malthusians, have been tempted to predict the future, and also by implication to predict future scientific developments. Both pessimistic and optimistic views have been stated. I do not think that we need worry now about future scientific developments in agriculture, because science is so far ahead of its application to the field of production. The potential food production in the world is tremendous if we could only apply our present scientific knowledge. For instance, it has been demonstrated that if one merely takes to an Indian village the technical 'know-how,' and the farmers are persuaded to apply it, production can be raised by more than 40 per cent in about two years. If this could be done over the whole of India, it would raise the level of nutrition, which

is at present woefully low for health and energy, to a standard equal to the average of Western countries and also provide a surplus. By this I do not mean to imply that we have all the answers to the production problems in India or, for that matter, in any country.

If a farmer were to ask us to predict when we would be able to provide the answer to all his problems of production, I think that the answer would have to be *never*. Not because we do not have hope of solving at least a large number of the present problems some time in the reasonably near future, but because, as we gain more information in each field, we see further avenues along which we can progress ; also because each new technique tends to present us with new problems.

For example, the scientist breeds a higher-producing wheat. It lodges : so he has then to breed it with stronger stems ; it is susceptible to a disease : so he has to breed a resistant variety ; and so it goes on. New diseases and pests appear. Locally there is the comparatively new problem of eel worm in potatoes. The scientists are working on it and there is no doubt that eventually they will solve the problem. Whether it be an alteration to the cropping system, a treatment of the soil, or even perhaps a new resistant variety of potato, I for one would not like to predict. Again, in recent years, we have become aware of two new diseases in pigs. I say new, because we were not aware of them before, although it is possible that they existed without our knowledge. The causal organism in both cases is a virus. It is possible that it has been present for a long time, but only recently acquired the virulence which makes it of economic importance ; or perhaps it is only recently that it has developed an ability to spread and so draw our attention to it. Prediction is uncertain, therefore, in view not only of the problems which confront us today, but also of those new problems which may turn up and of which we are not yet aware.

To predict the discoveries or developments in a particular field is well-nigh impossible until such time as there is sufficient knowledge on which to base the prediction, and at this stage the statement may not deserve the distinction of being referred to as a prediction. For instance, until the plant breeder discovered the techniques of producing hybrid strains with the resultant increased production, it would have been impossible to predict the increased production from, say, hybrid maize. Once the hybrid maize had been produced and tested, a reasonable prediction could be, and actually was made, that it would revolutionise the production of maize. But that prediction was made, not on the scientific discovery, but on its application.

There is another type of prediction which may be made, and that is what one might term a 'qualified prediction.' This also is based on knowledge already acquired. An example of this is a statement made by Dr. Hammond in his Presidential address to Section M (Agriculture) when discussing the use of hormones and superovulation and the transplantation of fertilised ova. (See *Advancement of Science*, No. 38, pp. 237-242.) He said : 'As soon as the problem of the technique of transplantation in the cow has been overcome, it will be possible with the modern jet plane to export anywhere in the world whole herds of pedigree cattle as fertilised eggs for as many shillings as it now costs pounds for a single animal.' This again is a prediction based on the practical application of knowledge already gained. It is qualified because it is not a practical proposition at the moment, the problem of the technique of transplantation in the cow not having yet been perfected.

An example of a qualified prediction which was fully justified was the statement that Myxomatosis, the disease which kills wild rabbits, would reduce the rabbit population in Australia 'if it could be spread.' Quite a number of years of work and a lot of effort were vainly expended trying to spread the disease, and it was considered by many scientists that it could not be done, However, Nature provided the answer in the form of a plague of mosquitoes which carried the disease far and wide. The results have been as predicted. Pastures are improving, and already there has been a great increase in the production of wool and meat. But that is not the end of the story : the scientists are warning the farmer and the grazier to use every means possible to destroy the

few surviving rabbits since these animals may breed a strain of rabbits resistant to the disease, or perhaps the virus may lose its virulence or killing power, and thus act as a vaccine to protect the rabbit against a virulent strain of the virus. It is just possible that this method of biological control may be as effective as the cactoblastis moth was in returning millions of acres of fertile country to crop and animal production. But the history of animal diseases indicates otherwise. There are no diseases, affecting only one species, which cause 100 per cent mortality. If there were, they would automatically wipe out both the species and themselves.

From the history of science we can gain quite a lot of knowledge on its predictability. Generally speaking, the individual discoveries are not predictable until sufficient knowledge has been accumulated. There have been, however, many cases where we have had sufficient knowledge on which a prediction could have been made, but in which we failed to realise the significance of the information we had. Many cures classed as ' Old Wives' Tales ' have since proved to be of scientific value. These cures were discovered by somebody not a scientist, but with a faculty for observing and appreciating the implications of what was observed in a scientific manner. I was taught by an old Danish farmer in Australia to give the cud from a healthy cow to an ailing cow, to overcome what he called ' after-calving sickness.' This was many years before the scientists demonstrated the importance of the bacterial flora in the rumen of the cow, and the need for correction at certain times, particularly soon after calving. The Danish farmer had been taught by his grandfather, and probably no one knows who first discovered the value of reinfecting the rumen with the normal bacterial flora. How it was discovered must also remain a mystery ; perhaps it was as simple as a farmer taking pity on the sick cow not chewing her cud, and giving her a cud from another cow to start her off. He did just that, perhaps ; it worked, so he tried it again, thus proving the experiment. There are many other examples of which everyone is now aware : strong cold tea for burns, and the subsequent use of tannic acid ; mould on a whitlow and penicillin, etc. There were and still are many methods used which have no value, and if the originators had used scientific methods of controls and checks to test the effectiveness of the treatment, they would have soon learned that the trouble was cured ' in spite of ' instead of ' because of ' the treatment.

This leads us to those discoveries already classed as ' happy chances.' These are the happenings which capture the imagination of the young scientist and the public, both of whom give due honour to the scientist who has made the discovery. They are not considered as flukes, and rightly so, because there must be many such ' happy chances ' occurring all the time, but not to the man who has the knowledge and awareness to realise the significance of the happening. How many thousands of us noted the effect of moulds on the bacteria we were culturing ? How many people noted that the dairy farmhands did not get smallpox before Jenner recognised its real significance ? Ramon's discovery of the effect of formalin in detoxicating toxins without affecting their antigenicity is another example ; this particular discovery opened up a whole new field for us in the preparation of immunising agents. This type of discovery could not have been predicted, but the recurrence throughout history of this sort of discovery makes it safe to predict that there will be more of them.

Finally, there are those problems which have been solved as a result of prolonged and exacting work. Such work, though less spectacular in its execution, may be just as spectacular in its results. In this group must be included a great deal of what is termed basic or fundamental research ; the research which opens the door, just a little more frequently, to give us just a glimpse of what lies beyond, or which suddenly throws the door wide open and gives us a wide view of new fields. Such knowledge sometimes makes it possible to predict further discoveries or new applications of science, but even so, when we do get through the door, we so often find that what we thought we saw was really something quite different.

It is evident from these few examples and ideas that to predict the scientific answer to any one particular problem is most

difficult until such time as we virtually have the answer. Take a problem in which the Food and Agriculture Organisation is particularly interested. This is a disease of livestock which costs the world many millions of pounds of livestock products, and, because in many parts of the world animals are used for draft purposes, also costs a great deal in crop production. The Animal Production branch of F.A.O. has followed as closely as possible the research which is being carried out on this disease in various parts of the world. A scientist from Australia was working in the U.S.A. on fundamental research on the protein composition of the bacteria responsible for this disease. His work revealed new information, which, coupled with the history of the disease and problems of vaccine manufacture, indicated that this discovery might give us the key to the production of an efficient vaccine. One of the countries in the Far East had asked F.A.O. to provide an expert to assist them in the problem of controlling this particular disease by the production of vaccines, so the Australian scientist was recruited to help the government, and at the same time to investigate the practical application of the new knowledge he had acquired. Laboratory experiments now indicate that we may have the answer, but laboratory experiments, even when they involve the use of a few hundred bovines, do not prove the effectiveness of vaccines for practical field use. Large-scale field tests are now under way and in this manner we hope to have the answer, either yes or no, in the near future. I say 'we hope' to have the answer, not that we *will* have the answer, because there are so many possibilities : that the test may not be conclusive, freak weather conditions, etc., etc. Although the evidence is reasonably strong, it would be unsound and unscientific to predict that we will have the answer, but probably what is more important still, it would be unfair to the farmer and it would reduce his confidence in the scientist. This confidence in the scientist is essential in the field of agriculture if we are to continue to increase production to meet the increasing needs of the world.

While all the evidence indicates that it is not possible to predict individual scientific discoveries, as a rule, I think there is justification for belief in predictions in general. Both the total product and the efficiency of agricultural production have increased steadily through the centuries, and in particular during the last eighty years. This has been made possible by the application of new scientific knowledge in all fields of science : engineering research improves the efficiency of farm implements ; research by the chemist makes it possible to apply artificial fertilisers, and so on. There is every reason to predict that, as new scientific knowledge is made available, there will be further increases in both the total production and the efficiency of production. Perhaps this should be a 'qualified prediction' : qualified by 'provided facilities are made available to the agricultural scientist' and, a further qualification, 'provided facilities are made available to acquaint the farmer with the improved methods and techniques.'

The need for more facilities for the scientist and an improvement in the advisory services is, of course, greater in the less-developed countries. Potential agricultural production in the tropics is tremendous and the expenditure of effort on research in this area would probably give a greater return in production than elsewhere. With 68,000 extra people in the world to feed every morning of the year it is essential that we improve our production. What is probably more serious is the low plane of nutrition on which a large proportion of the world's population are existing, and it does not need a very wise man to predict that there will be no peace until such time as conditions are so improved as to provide these people with a diet that is adequate for health and well-being. As I said in the beginning, the immediate problem is not the need for scientific research to increase production in these less-developed areas of the world, so much as research in the methods of making scientific knowledge available to the farmer. Perhaps the research should be on how to convince governments that they should make more facilities available for this work, so that the farmer can be taught to apply the uniformed techniques already known.

The world food survey carried out by

F.A.O. indicates that the rate of improvement in production is but very little more than enough to feed that extra 68,000 each morning. The levels of nutrition (inadequate as they are in many areas) are being maintained. But this is not enough. Admittedly, famines are mainly a problem of distribution, but semi-starvation (low level of nutrition) of great masses of people is a problem of production. Finally, this brings me to the functions of the United Nations agencies and their place in the scientific world. Time does not permit me to go into detail of the work which is being done by the hundreds of experts helping the less-developed countries to apply the new methods and techniques, which have been developed and tested in the advanced countries. Their activity presupposes two things : one, that the secretariat is aware of the problems, and two, that they are in touch with and fully aware of the progress of scientific research. Here we can make another of our qualified predictions, or, rather, I should say that the agencies are in a position to make predictions, not on scientific discoveries, but on the application of such discoveries. With a knowledge of the problems on a world-wide basis, it is the responsibility of the agencies to channel those methods and techniques to the countries with the specific problems, keeping in mind one essential point, and that is that the environment (in its broadest sense) in each country probably is very different. In other words, each newly developed method or technique in an advanced country may need drastic modification before it can be practically or economically applied in a less-developed country. For instance, it is impossible to change in one step the harvesting of crops by the method using the ancient sickle to the modern method of the combine. The methods of farming, the educational standard of the people and their knowledge of machinery, make such a sudden change impossible, but a great deal can be done to increase the efficiency of production by the introduction of such a simple implement as the scythe, and the mechanical mower is a big advance on that.

I would say that the duty of an international agency of the United Nations is to act as a unit to interpret the problems of each country on a world basis and make this available to the scientist, and at the same time to evaluate the work of the scientist in relation to the world problems and to assist in the practical application of the new techniques and methods in solving the world's problems. This can only be done if scientists the world over are willing either directly or through scientific associations to co-operate and act in an advisory capacity to what must remain as a skeleton staff attached to the control body of the agency. The agencies would thus serve, through the scientists, to guide the development of science along lines which one might predict, for the service of mankind.

AFFORESTATION OF SAND DUNES

At the Association's Meeting in Liverpool, Section K (Botany) and Sub-Section K* (Forestry) held a joint session on the morning of September 8, at which papers illustrative of sand dune afforestation and of the botany and ecology of the Freshfield dune system were given, together with a film of the Culbin sands, Morayshire. Two of the papers given are printed below. The first is by James Macdonald, C.B.E., Director of Research and Education, Forestry Commission, and the second is by Mr. P. Thaarup, Director of the Dune Service, Copenhagen.

Tree Planting on Coastal Sands in Great Britain

by

James Macdonald, C.B.E.

Although the Danes and the Dutch were probably the pioneers in the work of fixing coastal sands, there is no doubt that the operation of this kind, which has chiefly caught the imagination, is the great project carried out by the French in the Landes of Gascony with which the name of Brémontier is associated. There, on the open, and often stormy, coast of the Bay of Biscay, that vast undertaking gradually took shape during the eighteenth and nineteenth centuries; the littoral dune, which runs along the Gascon shore for more than a hundred miles, was built and stabilised, the sand on the landward side was fixed and the great forest of pine created. Although nearly two million acres of forest were formed, the dunes, in the strict sense, which were treated, covered about a quarter of a million acres and it was on this area that most of the engineering and silvicultural skill had to be employed. The results, partly because of their success, partly because of the scale on which that success was achieved, have had an influence far beyond France and foresters in many countries have used the same methods, or modifications of those methods, in dealing with problems of the same kind.

This work, in France and in other countries, has shown how a sandy coastline may be stabilised and moving sand arrested, so that it is no longer able to spread and endanger more valuable land. This, of course, can be done with other forms of vegetation than trees but the great advantage of trees is that they are the only crop which can be grown successfully on sands which one dare not break with the plough. Under suitable management, the tree crop may be renewed and perpetuated without laying the sands bare and it is fortunate that, on many coastal sands, natural regeneration of pines and other conifers has been possible and often prolific.

Afforestation on these sites must be regarded essentially as a work of land reclamation; protective, in that it fixes sand which is moving and prevents its spread in unwanted directions; productive, because the crops of trees, which play so important a part in the protection, yield substantial, and often very great, returns, from land which is otherwise incapable of producing anything of value.

During the last thirty years, extensive areas of coastal sands, both fixed and moving, have been planted with trees in Great Britain and, in all, about 10,000 acres have been afforested between 1922 and 1952 at various places on our coasts. Of them the most important are Culbin, on the Moray Firth, Tentsmuir on the Firth of Tay, Pembrey on the Glamorgan coast and Newborough Warren in Anglesey. But we must not forget that, although most of our sand afforestation is of recent development, we have to go back more than a century for the record of the first attempts in this direction. The oldest is probably the planting carried out

in 1839, on the estate of Kincorth, on the edge of the Culbin sands on the Moray coast but this was followed quickly by much larger plantations, made between 1840 and 1842, by the proprietor of the neighbouring estate of Moy who planted almost three hundred acres of sand (4). Later, the Chadwicks, owners of another Culbin property, Binsness, created plantations which have survived into our time. Meanwhile, the celebrated plantation at Holkham on the north coast of Norfolk was developing. This started, about 1850, with an unsuccessful attempt at direct seeding, but planting was soon resorted to and it continued over roughly the next forty years. Similarly, work which began in 1887 was proceeding on the Ince Blundell Estates near Formby, and various small plantations were being made at different points along the coast. Many of those plantations, of course, were formed on the sandy flats which so frequently are found on the landward side of the dunes and others were established, often with some difficulty, on dunes which had already been fixed, or partially fixed, by the natural development of vegetation. Very little was done on the mobile sands of unfixed dunes.

Most areas of coastal sands may be divided into several tolerably distinct regions. On the landward side there is usually a stretch, often an extensive stretch, of flattish fixed sand, generally covered with a vegetation in which *Calluna* is dominant although the herbage in some places is composed mainly of grasses. Frequently, the sand covers old pebble beds and, not infrequently, there are wettish areas, the result in many cases of the interruption of drainage caused by the movement of sand on the seaward side. In these fixed flats the water-table is often relatively high. Ovington (7) in his account of this type of sand at Tentsmuir, in Fife, found the water-table at just under 3 feet and indicated that it varied in depth at different seasons of the year, rising, probably to about 18 inches at certain times.

These fixed sands of which I have been speaking are generally quite easy to afforest and, as they are usually sheltered from the blast of sea-winds by the intervening dunes, a greater variety of species may be planted than can be used nearer the sea. Pines are usually employed as a first crop, but Douglas fir can be grown with some success, while trees like sycamore have succeeded in one or two places and might find a more extended use in the future. Birch is often abundant in places, having seeded itself, but it is rarely of good form. Although there is, generally speaking, no difficulty with these sites, beds of pebbles and shingle are difficult to plant and the young trees may suffer heavy casualties. One must be careful, too, with the areas of wettish ground. When one approaches them first, they often look highly suitable for moisture-loving species such as spruce, poplar or alder but the growth of the trees on the surrounding, less moist sites, leads gradually to a fall in the water-table and to the drying up of the patches which were originally wet. After a few years, the sites which looked so suitable for spruce, poplar or alder, look suitable no longer. It is usually best to leave such areas unplanted until the effect of the plantations round about has been revealed. It will then generally be found that they can be planted successfully with pine.

Moving towards the sea, one passes into a region, irregular in topography, composed of dunes of various sizes, interspersed with sandy or shingly flats and occasionally with winter lakes, which fill with water in the winter and dry out in summer. The dunes have been fixed by the growth of *Ammophila*, which is usually abundant on the higher parts, and also by *Carex arenaria*, which is generally more plentiful round the bases of the dunes and which extends over the sandy flats where it is accompanied by the creeping willow. When these dunes are firmly fixed, they can be planted with pine without any great difficulty ; almost invariably, however, they contain patches where the surface has been broken by wind or more likely by the action of rabbits or of human beings. These broken surfaces tend in time to develop into blow-outs from which the sand may start moving again and they must be repaired immediately before planting starts. This is usually done by thatching. The flats between the fixed dunes do not look difficult to plant but they are subject, rather surprisingly, to

frost damage in the late spring and early autumn and trees like the Corsican pine sometimes suffer injury when planted there. The winter lakes present no problem because they are usually full of water in the planting season. On several occasions they have been emptied of water by the cutting of drainage channels but this is a method which is not always easy because any open ditch keeps filling up with sand. Experience at Culbin has shown that these winter lakes dry up after the surrounding dunes and flats have been planted for a number of years.

The last region is adjacent to and merges with the beach : the region of moving sand. This may occupy a very small proportion of the total area of sand as, say, at Tentsmuir, or it may cover a relatively large area as at Culbin, where great bare moving dunes, a hundred feet high, used to dominate the landscape. Among the moving dunes there are flats, depressions and sometimes winter lakes, but the topography is excessively fluid. Patton and Stewart (8, 9), in their accounts of the Culbin Sands, give a good account of the changes which took place there over a period of ten years.

In this region, the problem is two-fold. The supply of moving sand must be cut off at its source ; the mobile sand already on the area must be fixed and prevented from moving further. The sand may be coming in at approximately right angles to the coastline ; it may be coming in at a more acute angle ; in the exceptional case of Culbin, it is travelling parallel with the coast. Where the sand is coming in direct from the sea, it is necessary to build a coastal dune to dam the incoming sand ; in an area like Culbin no coastal dune is necessary.

The conditions which are usually required for successful resistance to sand invading from the sea are a gradual shelving beach on which the waves can break their force and, behind it, a barrier dune to stop the incoming sand. This dune is built up by a system of fences or semi-permeable barriers of various kinds, which trap the sand and allow it to pile up, and, by careful manipulation of those barriers, the dune can be built to the required shape, and size.

The littoral dune is then fixed by the planting of *Ammophila* and once fixed, it must be carefully maintained.

The planting of marram grass is the classical method of fixing mobile coastal sands and we have seen it in practice within the last few days near Formby Point. It has been used in Wales ; it was used in the early days of the Forestry Commission's work at Culbin (1). But for some time now it has been superseded by thatching which gives a much quicker effect. There is nothing new about thatching. It was being used as long ago as 1804 by Déjean in France to cover his sowings of maritime pine and it was used in the Culbin district in the middle of last century. But it is an extremely expensive method unless there are supplies of thatching material—branches of conifers, birch branches, broom, etc.—ready at hand and its conveyance to the places where it was wanted was very difficult before the days of the tracked tractor.

It is customary with us to plant immediately after thatching. Direct sowing of tree seed, which has been used in other countries, has been tried in Great Britain but has generally been unsuccessful and is nowhere used now.

The best examples of afforestation behind a littoral dune are found in Wales where there are two dune forests, one at Pembrey on the Glamorgan coast and the other at Newborough Warren in Anglesey. Both places face the open sea and the exposure is severe. The work at Pembrey, which has been described by Ryle (10), is reasonably successful and there is good growth of a number of species where there is some shelter. That at Newborough Warren has not been described, so far as I am aware, in any published paper but it is in a most interesting stage. Because part of the area had been used, until quite recently, by one of the service departments, the coastal defence had broken down, sand poured through the gaps, some of the fixed dunes further inland became eroded by wind and a small area of young plantations was smothered by sand. Now the dune is being reconstructed and the sands within fixed by thatching. The operations there are well worth study for they show what difficulties can arise when there is any breach in the main defences.

The burial of plantations by moving sand has occurred several times in the past at Culbin. Grigor in his *Arboriculture*, published in 1868 (3), describes how one of the earlier plantations was engulfed by sand with little more than the crowns of the trees protruding and with some of the lower side branches, outcropping, so to speak, round about. He tells how the side branches of larch rooted in the sand below the point of emergence, so that the shoots gave the impression of transplants which had been heeled in on the sand and forgotten. This phenomenon was not observed in Scots pine.

More recently, as a result of felling during the first war and of subsequent fires, a moving dune partly buried a plantation of Scots pine in the same forest, covering 10 feet or more of the stems of the trees. When some of those trees were excavated, a good many years later, it was found that below the level of the sand the stems of the trees were considerably smaller in diameter than they were above it. Above the new ground level, diameter growth seemed to be quite normal ; below it, diameter growth was very much reduced. Again, when the sand was removed, the trees seemed to make great efforts, by laying on extra wide annual rings to increase diameter growth in the sections which had been covered for so long. Similar conditions have apparently been created artificially during some work on stem stresses in Australia when trees were embedded in concrete with similar results. It has been suggested, that as the sand piled up, the point of stress proceeded up the stem also and the tree responded by laying on less wood below ground where there was no longer any particular need for strength. As soon as the sand was removed the point of stress returned to its original level and the trees made desperate and successful efforts to accommodate themselves.

The plantation at Holkham, in Norfolk, to which I referred earlier, is of interest because it demonstrated the value of Corsican pine as a tree for planting on sands by the coast. Actually, in that plantation, a good many different forms of the European black pine are found but there was enough of the true Corsican form to show its advantages. One of the difficulties which foresters have in this work is the limited number of tree species which are available. We require a tree which will tolerate a dry site, which does not mind salt spray, which will resist exposure to the sea. So far, the Corsican pine has proved the best choice, even in the north, but it is doubtful whether it is quite suited to cope with extreme exposure. This was suggested by Ryle in his paper on the plantations in South Wales ; we saw the other day, at Formby Point, how Corsican pine had been blasted when it was suddenly laid open to extreme exposure.

That visit to Formby Point was interesting for two other reasons. It showed that those in charge of dune plantations must keep a close watch on what is happening to the coast itself and must be ready to re-arrange their defences according to changes in the behaviour of the sea. It is not always easy for foresters to determine exactly what is happening to the coastline and it is here that books such as that recently produced by Dr. Kay Greswell (2) can be so valuable. The other point which was clear enough at Formby Point is the damage which can be done by the movement of large numbers of people over and among the dunes.

The development of woodlands on such peculiar sites is bound to fascinate everyone who is interested in biology or natural history. It is interesting, for example, to see the appearance in the oldest plantations at Culbin, of such characteristic plants of the pine woods as *Goodyera repens* and *Pyrola*. The crested tit is also present and as long ago as 1922 J. W. Munro (5) drew attention to the gradual building up of a typical population of forest insects. We all know how quickly a plantation of Scots pine in the north of Scotland acquires the characteristics of the native pine-woods, so much so that it is often quite impossible, in the absence of written record, to distinguish between an old plantation and an authentic relic of the ancient pine-birch forest. To what extent the Scots pine at Culbin, which is on the edge of a pine district, will follow that trend we do not know. Nor do we know what will be the result, in this sense, of the replacement of Scots pine by the Corsican. These new plantations on the

sands in England and Wales as well as in Scotland, seem to me to offer a most valuable opportunity for the study of the effects of imposing an artificial and often alien population upon sites of such marked peculiarity.

The planting of newly-fixed sand dunes gives one an unequalled chance to study a soil in the making. We are fortunate indeed that the Macaulay Institute at Aberdeen have interested themselves in this matter and have been able to carry out a considerable body of work at Culbin and at Tentsmuir, first by Ovington (6, 7), and latterly by T. W. Wright (11).

In his latest report, Dr. Wright shows that on the sands at Culbin, profile development by leaching begins as soon as the sand is stabilised. Thatching is beneficial because it adds appreciable quantities of phosphate, in which the sand is sadly deficient, potassium and manganese. He confirms the findings of Ovington that the growth of the trees starts a movement of nutrients which are taken from the sand at depths depending on the rooting of the young trees. A proportion of these nutrients is returned to the surface in the litter. The return is smallest with young pines but once they have grown and formed canopy the surface soil begins to be enriched from the litter and under the older pines the surface soil has a considerably higher nutrient content than the bare sand, although, naturally, there is still a deficiency at rooting depth.

Another point of direct practical importance arises from the studies of moisture which were carried out at the same time. The generally good results of planting on dunes seem to be accounted for by the fact that the bare dunes, in summer, do not dry out to a depth of more than 2 inches. The availability of moisture to the newly planted stock, no doubt accounts for the relatively small numbers of failures which are often experienced. The trouble may come later. The growth of trees on the dunes removes large quantities of water through transpiration and very dry layers occur in the sand round the roots of actively growing trees. These dry layers have been found with all trees about the beginning of July.

This raises an important practical issue and it has been found that moisture deficiency in these sands can be reduced by heavy thinning, provided the surface of the soil is protected against wind erosion by an adequate cover of thatch.

On a good fertile soil a forester may do many things, undesirable on general grounds, without any great harm coming to the forest, but on the sands of which we have been speaking to-day, there is little margin of safety for that kind of adventure. It is all the more important, therefore, that we should understand the processes which are at work so that we can correct in good time any practices which are likely to lead to trouble. In this brief review I have not attempted to give any detailed account of the forestry work which is going on at the present time on coastal sands in different parts of the country. That would have taken too long. What I have tried to do is to show that in this small corner of British forestry there is much that is of interest to the scientific public, that there is room for more research and that that research is needed as a basis for sound practice. In this attempt, I hope that I have not wholly failed.

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The Afforestation of the Sand Dunes of the Western Coast of Jutland, Denmark

by

P. Thaarup

Dunes have probably existed in Jutland as long as the country has had its present configuration, but the violent and harmful drift of the sand seems only to have become marked in the sixteenth and seventeenth centuries. Denmark has probably suffered more in proportion from drifting sands than any other country in Europe. The dune areas, that is both the dunes proper and the adjacent plains, cover about 74,000 hectares (185,000 acres) or 1·7 per cent. of the land area.

The first recorded legal provision in the fight against the drifting sand is the royal resolution of the year 1539 which prohibited the removal of any vegetation. Further ordinances followed, but the people were too poor to be able to do without the grazing and fuel which the dunes provided, and the damage continued. There is little doubt that human stupidity was an important cause of the drifting of the dunes on the west coast of Jutland.

Gradually the dunes conquered more and more of the arable land so that the houses, farms and even whole villages were destroyed : the people had to move and churches are known to have been moved eastwards more than once. The sand drifted either as big wandering dunes which advanced some 8 to 10 yards a year, or else as long tongues of sand which, during violent gales, would extend in thin layers over the fields. Sand carried by the wind was lifted high in the air and fell in a fine powder over the cornfields, leaving only the ears of corn exposed.

Not until 1779 was an experiment started on part of the coastal area to protect the dunes and reduce the drift of the sand. This experiment had legal sanction and was a success, and in 1792 the law was extended to the whole western coast of Jutland. This most important law was elaborated on the basis of a paper written by the botanist Viborg who was a professor in Copenhagen. After approximately 25 years' work the drifting sands were brought under control, but it was still very expensive to keep the sand at rest. At first the inhabitants themselves had to do all the work without payment, but after 1852 the State, counties and parishes bore a good deal of the cost.

Under present laws all the sea dunes along the western coast of Jutland are protected, which means that the owners are prohibited from using the dune areas for anything at all. In return the State, counties and parishes pay all the expenses incurred in supervision of these areas. Not only the sea dunes but large areas, especially those with high dunes further inland, are protected and supervised by the State Dune Service.

Afforestation

In 1788 Prof. Viborg had recommended tree-planting, partly to bind the sand for ever and partly both to provide fuel and to exploit areas otherwise waste and useless. However some small-scale experiments between 1820 and 1830 failed, and tree-planting was given up as impossible. In 1851 Prof. Bjerring proposed that a new attempt be made : so in 1853, exactly a 100 years ago, the first appropriation of funds for dune planting was made and ever since then afforestation has been continued. To-day the Danish Government owns 41,000 hectares (102,000 acres) of dunes of which 30,000 hectares (75,000 acres) have been afforested.

During the early years, the planting of trees was mostly experimental. Many different species were tried, both coniferous and broadleaved. The planting was done in the bare unfixed sand but it soon became evident that this method was not successful. After some years of experimenting one of the workers developed a method by which the sand was covered with a layer of cut heather before planting. This kept the sand stable until the young trees had taken root. After a few years it was clear from the experiments that only a few species were able to grow, namely Austrian pine, mountain pine and Scots pine. The last-named species was not very successful, and the Austrian pine looked the most promising, but after about 15 years it was heavily attacked by a fungus (*Crumenula pinea*) and this completely destroyed the young crops. This left only the mountain pine

and with this tree the main part of the dunes was afforested. Here and there on better ground small areas of Scots pine, Sitka spruce and silver fir were introduced. During the first 25 years the larger dunes only were afforested, but adjacent areas and plains situated between the dunes, as well as the other areas with less sand, were later included in the forest. In these places the conditions for tree growth were considerably better.

The faces of the dunes continually receive sand from the beach which makes it essential to control drift by the planting of marram grass. Afforestation of these areas is impossible. Behind the sea face lie the grey dunes where the plantations are situated and here the soil can be classified as follows : (1) Large dunes consisting of very poor sand. The sand is especially infertile where the dunes have drifted a long way, which may often be 5 to 7 miles. During this process the sand has become more and more leached. (2) Dunes which have drifted over more fertile ground, for instance fields on clay soil. In these cases the sand has often absorbed some of the clay material and provides a relatively good soil for forestry. (3) The dune heath, i.e., the plains between the dunes. These areas are often covered with heather and grass. The ground water-table is near the surface. In such places even if the sand is poor, growth may be tolerably good, but drainage is necessary to lower the water table to at least 30 to 40 ins. below the surface. (4) Fertile soil especially clay soil covered by a layer of drift sand which is not too thick to prevent the roots from penetrating to the original ground. Such places are ideal for forestry and wind resistant trees such as Sitka spruce are able to grow well.

In the last 30 to 40 years the policy has been somewhat changed, and now afforestation is only practised where the conditions for forestry are tolerably good. Trees are being planted to produce timber, and to provide employment, and not solely for the control of drifting sand, which is more cheaply accomplished by planting with marram grass and by covering with cut heather.

The climate of the west coast of Jutland is, of course, strongly affected by the sea. The mean annual temperature is from 44·6° to 45·5° F. and the rainfall is low, being about 24 to 30 ins.

The Species Employed in Afforestation

Mountain pine (*Pinus mugo*). In the dune plantations the mountain pine is able to thrive on any except the dampest soil, but it cannot stand exposure to wind and salt spray close to the sea. Its growth on exposed slopes is extremely slow, 70- to 80-year-old trees being only 30 to 40 ins. high, whereas on the lee side of the dunes at the same age they may be 15 to 18 feet. The slowly growing mountain pine is always healthy and still sound when a 100 years old. Where the object of the mountain pine forest is only to control the dunes, such slow development of the trees is of great value, but where marketable produce is also envisaged it has been found that the single-stemmed mountain pine of French provenance is preferable. It thrives better near the coast than the common variety, although the latter seems to be better on inland heaths. We get most of our seed of French mountain pine from Briançon in the Alps in the south-east of France, but unfortunately we do not know exactly whence the seed of our best stands was obtained. Consequently some of the best trees in Denmark have been selected and reserved for seed collection. We are also grafting twigs from these trees in order to preserve their individual quality for the future. Provenance experiments have been made with the seed of the single stemmed or upright mountain pine obtained from several places in France and Switzerland. On the poorest sites the mountain pine will be retained and regenerated naturally.

Scots pine (*Pinus sylvestris*). Scots pine is used to a great extent on the poor dry soils and it is the best species to plant after a first crop of mountain pines. Like all conifers in Denmark it is an exotic, and its history has been characterised by sudden changes in fashion. When the first planting of the heaths took place about 1790 a great deal of Scots pine was used, though unsuccessfully. After a few years the plants died, probably because a seed of unsuitable provenance had been used ; as a result

the planting of Scots pine was prohibited. Since provenance experiments have been carried out, Scots pine from both Scottish and Norwegian seed has been used. Although Scots pine had been given up in the plantations on the heaths it was still used on a small scale in the dunes. Some small stands of pine still remain and are most valuable on account of the information they give about the growth of the trees. Some sample plots indicate a mean annual increment for 60-year-old crops of 45 to 60 cubic feet per acre. Nevertheless these plots suffered severely from exposure in their youth. The pine now planted after the mountain pine will do better. This may seem a small return, but considering the soil and the climatic conditions this yield is considered quite satisfactory. The mountain pine is felled in strips 12 to 15 yards wide, running north and south, and these are planted with Scots pine. All the brushwood is left on the ground, a most important factor on dry soil. The crop nearly always does well, and when it has grown to 3 to 6 feet the strips are widened and so the process goes on until regeneration is complete.

The best sources of Scots pine seed have not yet been determined. Seed from Scotland produces the most vigorous stands but the trees of Norwegian provenance have a better stem form. They all suffer seriously from exposure and almost every spring the two-year-old needles turn brown, giving the trees a most miserable appearance. Hence Scots pine is rarely or never planted in the dunes : when used on the heaths it is always mixed with mountain or Austrian pine to protect it in its youth.

Lodgepole pine (*Pinus contorta*). During recent years a good deal of *Pinus contorta* has been used on the heath. It grows quickly in youth and soon reaches a good height, if not attacked by insects which often deform the stems. The yield is 50 to 60 per cent. greater than that of mountain pine. The trees must be fairly widely spaced, between 1,000 and 1,500 per acre, but they do not continue to grow to the same age as mountain pine. Root rot and windfall are common. Nevertheless this species is useful in afforesting large areas where it is not desired to plant only one species. It grows quickly and gives a better distribution of size classes over the whole area. An annual increment of about 80 to 90 cubic feet per acre is expected.

Austrian pine (*Pinus nigra*). There is no doubt that Austrian pine is the most tolerant and wind-hardy tree on our plantations, but unfortunately it cannot be grown in stands. When the trees reach the age of 15 to 20 years they are attacked by *Crumenula pinea* and completely destroyed : only isolated trees withstand the attack. It is unfortunate that the climate of Denmark is too northerly for the use of Austrian or Corsican pine.

Sitka spruce (*Picea sitchensis*). The Sitka spruce is one of the most important species on the better soil. It is our most wind-hardy spruce, is easy to cultivate and is not liable to be damaged by deer. (Fortunately there are no rabbits in Denmark.) The first Sitka spruce was planted in 1885 and the good growth which resulted attracted a great deal of attention. However, only small areas were planted and these all on the better soil where a very good yield was obtained. When the regeneration of the mountain pine stands was started about 25 years ago they were often replanted with Sitka spruce, even on poor and dry soil. At first the results were most promising, but soon the plantations stagnated and it was evident that conditions were not suitable. In regeneration by strips the growth of Sitka spruce was at first good where planted among the brush but difficulties were caused when the strips were enlarged and the spruce would not grow on the soil which had deteriorated under the adjacent old mountain pine. We consider that Sitka spruce requires especially favourable nitrification conditions and if Sitka is planted now it is introduced underneath the old crop of mountain pine. It grows best on clayey soil covered by a fairly thin layer of drift sand which permits the roots to get through to the sub-soil. The yield is very good, a mean annual increment of between 210 and 250 cubic feet per acre being produced. Thinning experiments carried out by the Danish Forest Experiment Stations show that stands which are never thinned remain practically free of root-rot, whereas attacks follow a thinning and damage the

crops. At the timber producing stage Sitka spruce suffers severely from root rot and this has been followed by attacks of the beetle, *Hylesinus micans*. It is hoped that these dangers will not impede planting of this valuable tree, but the experience of the last few years makes us more critical in selecting places where Sitka spruce is planted. Regeneration of the old stands will not be easy, because of the adverse soil conditions under them and because they will be destroyed if opened up for underplanting. Natural regeneration occurs but the young plants die if the shelter wood is removed quickly.

Silver fir (*Abies alba* or *pectinata*). Owing to the doubt as to the future success of Sitka spruce it is expected that the use of silver fir will be extended in dune plantations. Some of the older stands show that the trees are able to thrive even on rather poor and exposed sites. Crops of silver fir are difficult to raise on account of its need for shelter during early years and its susceptibility to frost in the spring. Mountain pine is not generally able to give shelter for a sufficiently long period and more rapidly growing trees such as larch, birch or alder are often required. When established, silver fir makes a very good stand in spite of attacks by *Adelges*.

Other species. Douglas fir, *Abies grandis* and *Tsuga heterophylla* have only been planted in very recent years. The first two suffer considerably from the wind and it is intended to mix them with Sitka spruce. *Picea omorika* has not hitherto been planted but experiments will be made. Japanese larch has been planted a good deal during recent years. Broadleaved trees do not thrive well on account of the wind, at any rate not in stands, although beech, oak, birch and others are planted in small groups in shelterbelts in order to stabilise the forest and for amenity purposes. Shelterbelts are established along the exposed borders on the better soil but where only mountain pine is grown special shelterbelts are not necessary. A mixed stand is of course the safest kind of shelterbelt and should consist of species which resist exposure and have a long life, such as Scots pine, Austrian pine, silver fir and beech. A shelterbelt must also provide shelter near the ground in order to check soil deterioration. Thus *Prunus serotina* is planted on the outside: it is a wind-hardy bush able to thrive on fairly poor sites and it regenerates freely.

Conclusion

Forestry on the dunes of Jutland is far more difficult than in other parts of Denmark. This is mainly because of climate, and chief among the climatic factors is the extremely strong effect of the wind. This is evident in the sloping edge of the forest and of shelter trees growing in gardens. The shape of broadleaved trees is affected much more than that of conifers. However, the summer humidity of the air near the coast is higher than inland, and this may help the trees. The dune plantations are essentially protective forests and an economic return is not to be expected. Up till now their management has resulted in an annual deficit, at present approximately 600,000 kr. a year (approximately £30,000), but when all the better soil has been planted with the more valuable species, such as Sitka spruce, silver fir and Scots pine, it is possible that a surplus will be shown. Except for the mountain pine areas the mean annual increment is estimated at approximately 70 cubic feet per acre, but the success of the plantations must not be judged by economic results only. The effects of plantations in checking the movement of the sand and in providing shelter, employment and timber are essential to the well-being and economy of the western part of Jutland.

THE CONTINENTAL SHELF

At the Association's Annual Meeting in Liverpool, Sections C (Geology), D (Zoology) and E (Geography), held a joint session on Tuesday, September 8, 1953, on the Continental Shelf. Dr. L. H. N. Cooper, of the Marine Biological Laboratory in Plymouth, delivered a paper on the Celtic Sea and Eastern North Atlantic which dealt with the effect of fluctuations in Arctic climate upon vertical and horizontal circulation of the ocean with especial regard to changes in biological productivity in the neighbouring seas. Mr. Michael Graham and Mr. A. J. Lee, of the Fisheries Investigation Laboratory at Lowestoft, discussed the hydrography and natural history of the Bear Island Shelf. The third invited contribution to this symposium was a paper by Dr. A. E. Moodie, of Birkbeck College, on territorial problems associated with the Continental Shelf. It is printed in full below.

Some Territorial Problems Associated with the Continental Shelf

by

Dr. A. E. Moodie

This is an appropriate time to consider some of the territorial problems associated with the Continental Shelf because the United Nations International Law Commission, after five years of argument and counter-argument, has at last agreed to recommend the recognition, in principle, of qualified sovereign rights of a coastal State over the adjacent part of the Shelf. The Commission's findings will be submitted to the next session of the General Assembly for further consideration but it does not necessarily follow that they will be accepted and put into practice. Nevertheless, the announcement of July 20, 1953, that the legal experts have reached agreement on this complex and difficult matter, marks a stage in the solution of a problem which has bedevilled international relations for some years. What is more important is that the flow of unilateral proclamations on the legal status of the Shelf, which has continued unceasingly since 1945, may now end. Should the General Assembly accept the findings of its International Law Commission, order may be imposed on what have so far been chaotic conditions.

The International Law Commission defines the Continental Shelf as '. . . the sea bed and sub-soil of the submarine area contiguous to the coast but outside the area of territorial sea, to a depth of 200 metres.' This definition was adopted after protracted legal argument. The selection of the submarine contour of 200 metres (656·17 feet) as the outer limit of the Shelf is somewhat arbitrary, but in view of the legal implications of any territorial definition, a fixed line is necessary and the difference between 200 metres and the older convention of 100 fathoms can have but little significance in the majority of cases.

Inside this outer limit of the 200 metre line, a clear division is recognised by the international lawyers. The Territorial Sea (formerly known as the Territorial Waters) is separated from the waters of the Shelf for legal purposes. This decision is made irrespective of the width of the Territorial Sea and is necessary because all littoral States claim and exert full sovereign rights over these immediately adjacent waters as well as their sea bed and sub-soil. If the recommendations of the International Law Commission are adopted, all contiguous waters will be divided into two zones or belts. First, that of the Territorial Sea where full sovereign rights will be exercisable and, secondly, a normally much wider zone where qualified sovereign rights will be exercisable because, as the Commission advises '. . . it would be unwise to recognise full and entire sovereignty of States over the Continental Shelf.' Indeed, the Commission recommends that this

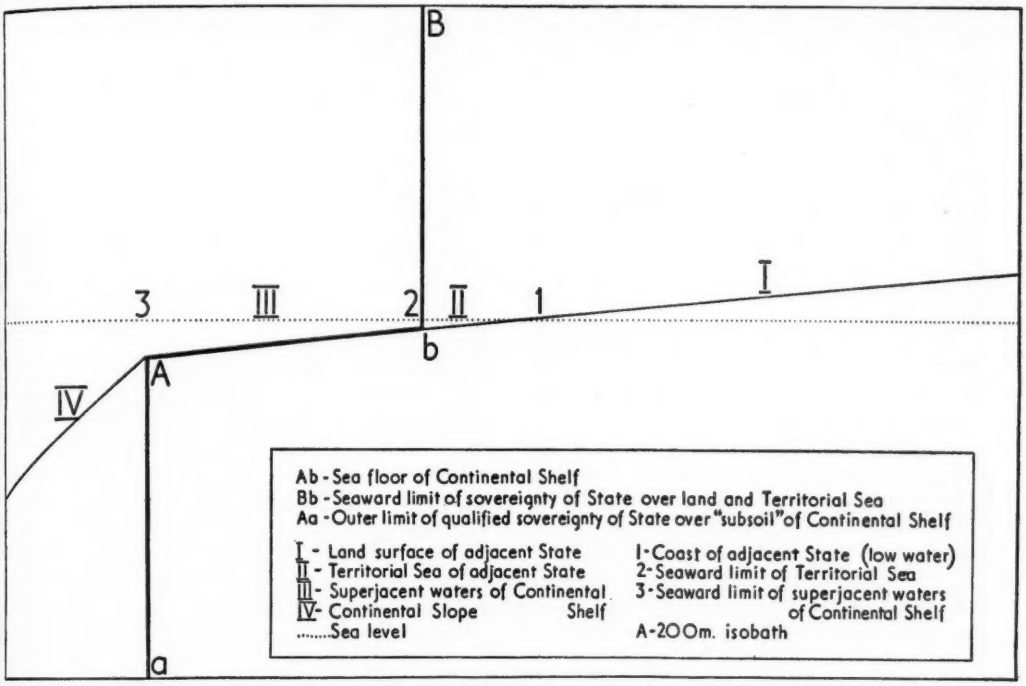
B
3
III
2
II
1
I
b
A
IV
a
Ab - Sea floor of Continental Shelf
Bb - Seaward limit of sovereignty of State over land and Territorial Sea
Aa - Outer limit of qualified sovereignty of State over "subsoil" of Continental Shelf
I - Land surface of adjacent State
II - Territorial Sea of adjacent State
III - Superjacent waters of Continental Shelf
IV - Continental Slope
.....Sea level
1 - Coast of adjacent State (low water)
2 - Seaward limit of Territorial Sea
3 - Seaward limit of superjacent waters of Continental Shelf
A-200m. isobath

Fig. 1.—Schematic cross-section of the seaward boundary of a littoral State.

zone should be recognised '. . . only for the purpose of exploring and exploiting its natural resources.'

The major difference in the legal status of Territorial Sea and Continental Shelf is that, in the first case, the State exercises, as before, complete sovereignty over sea floor, sub-soil and superjacent waters whereas, in the second case, the superjacent waters are not subject to the sovereignty of the littoral State. This differentiation is deemed to be necessary because of the doctrine of the Freedom of the High Seas, a legal concept which has been generally accepted for a long time and necessarily so because of the great importance of ocean-borne trade. Under the newly recommended policy, the High Seas, that is, all the waters between the outer limits of Territorial Seas, remain free to all their users and this applies to free passage as well as to fishing.

Certain implications arise from the recommendations of the International Law Commission which are of interest to geographers as well as to lawyers and others. The most important of these is concerned with the definition of maritime international boundaries.

Both international and national law recognise the necessity of boundaries for all States. For many purposes the world is divided into a number of more or less watertight compartments in each of which the State possesses full sovereign power. The seaward limit of this authority for a littoral State was, until recently, defined as the outer limit of its Territorial Sea. Such boundaries have not been demarcated for obvious reasons. In this connection, the point is often overlooked that all international boundaries are, in law, not lines but planes. On land, the demarcated boundary is a line and appears as such on maps but, in legal theory, the boundary extends upwards and downwards so that the State controls the air space above its territory, both land and sea, as well as the resources of the earth beneath it. Furthermore, States are very unwilling to allow any infringement of their rights. The development of the so-called Continental Shelf Theory introduces a completely new concept into the field of boundary making. If a certain amount of topicality be allowed, it may be called the 3D concept.

If the littoral State possessed full

sovereignty over the adjacent part of the Continental Shelf and its superjacent waters, then its seaward boundary would be a vertical plane passing through the 200 metre isobath. All the territory within this boundary would then be subject to the normal restrictions imposed on the State's land territory. This would mean a very great extension of the authority of the State and might well prove to be an insuperable hindrance to trade and fishing.

Should the International Law Commission's recommendations be adopted, the seaward boundary of a littoral State will have three parts :

(*a*) From the 200 metre isobath, it will be a descending vertical plane, the practicable depth of which will depend on accessibility to the natural resources of the sea bed and its sub-soil.

(*b*) At the outer limit of the Territorial Sea, the boundary will be a vertical plane passing through this limit from the sea floor upwards, *usque ad coelum* as the lawyers say.

(*c*) Between the line where plane *b* joins the sea bed and the 200 metre submarine contour, the boundary will be a gently inclined plane, the angle of inclination being dependent on the configuration of the Shelf.

These three parts are indicated diagrammatically in Figures 1 and 2.

Thus the new seaward limits will assume an entirely new character, involving three dimensions, height, width and length. The waters lying on the Shelf, but outside the Territorial Sea, will remain free to all users. These latter will, of course, include the inhabitants of the adjacent State and they will be at liberty to erect installations for exploring and exploiting the natural resources of the Shelf provided that in so doing they create no hindrance to the activities of the other common users. It is difficult to envisage any methods of demarcating

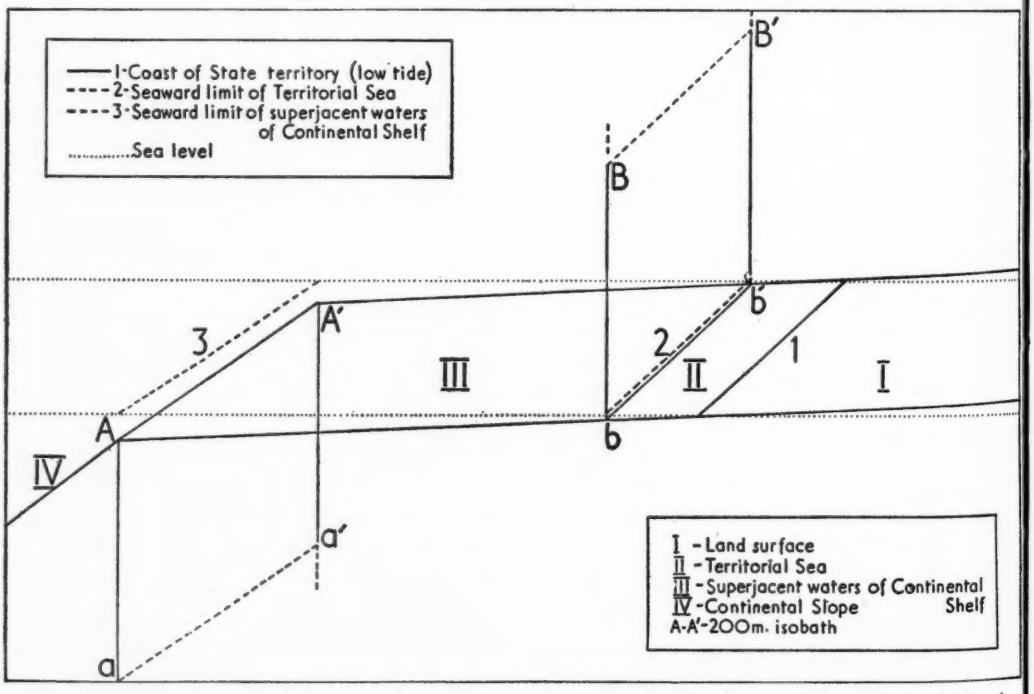

Fig. 2.—Three dimensional representation of the seaward boundary of a littoral State. Plane A A′ *a a′* represents the outer limit of qualified sovereignty over the sub-soil of the Continental Shelf. Plane A A′ *b b′* represents the floor of the adjacent Continental Shelf below which the littoral State will exercise qualified sovereignty. Plane B B′ *b b′* represents the seaward limit of full national sovereignty over the air space, and the waters of the Territorial Sea. Continued vertically downwards, it would indicate the outer limit of full sovereignty over submarine resources.

these new limits. Presumably the hydrographic charts of the future will be contoured in metres. The 200 metre isobath at least should be plotted but the old problem of navigators, especially fishermen, of knowing or, worse still, proving, when they are just inside territorial waters will remain. We shall probably still continue to read of trawler masters being fined £1,000 and having their gear and catch confiscated for fishing on the wrong side of part *b* of the new boundaries.

Further problems will arise where two or more States lie adjacent to the same Continental Shelf. ' On the sea coasts of the Continental Shelves of the world (including those of shallow seas such as the Baltic Sea and the Persian Gulf) there are more than 100 termini of international (including colonial) boundaries.' (1) In the case of the West European Shelf, it is clearly impossible for the rights suggested by the International Law Commission to be exercised by all the Atlantic littoral countries. Some bilateral or, preferably, multilateral, agreements will have to be made in such areas and atlases of the future will probably contain political maps showing the new limits of State sovereignties.

This new development in International Law appears to have had its origins in the *Declaration of Panama* which was issued after a meeting of the Foreign Ministers of the American Republics on October 3, 1939. Under this declaration, a security zone with a minimum width of 300 nautical miles was established off the coasts of the Americas. At that time, the Second World War had begun and the American Republics hoped to preserve their neutrality behind this extensive belt of water, a hope which was destroyed by the disaster of Pearl Harbour. Nevertheless, the strategic importance of adjacent waters was not forgotten as is shown by the Treaty of Petropolis of September 1947 which defined a region of reciprocal assistance extending from the North Pole to the South Pole. Such international arrangements can only be successfully implemented with the help of powerful naval and air forces and it appears that the United States Navy is undertaking a function like that long performed by the Royal Navy.

When the Second World War ended, the Declaration of Panama automatically became invalid because it infringed the rights exercised by nations in peacetime under the doctrine of the Freedom of the High Seas. In 1948, the General Assembly of the United Nations recommended to the International Law Commission that it should give priority in its discussions to the regime of the High Seas. This recommendation reflected the position, almost one of chaos, which since 1945 had grown out of numerous unilateral proclamations on the extension of sovereign rights over adjacent waters. In addition to security, the American Republics had become concerned with the exploitation of the economic resources of their adjacent seas. The two motives together found their expression in extravagant claims to control which are almost reminiscent of the famous Treaty of Tordesillas. (2)

This is hardly the place to analyse all the legal aspects of territorial rights over the Continental Shelf, but it may be useful to trace briefly what has happened in recent years. In June 1938, the State of Texas ' claimed dominion, with its use, ownership and jurisdiction, over its marginal waters to a gulfward boundary located 24 marine miles further out in the Gulf than the said 3 mile limit.' This claim was opposed by the Federal Government and the opposition was upheld by the Supreme Court of the U.S.A. In 1942, Britain and Venezuela made an agreement for the exploitation of oil under the Gulf of Paria which arranged for buoyed lines to mark the extension of sovereign rights in this shallow arm of the Caribbean Sea.

These early cases passed almost unnoticed but the spate of declarations which has perturbed international lawyers began with the Proclamation of the U.S. President on September 28, 1945. This so-called Truman Declaration, it is important to notice, was very carefully worded and only claimed sovereignty over the sea bed and sub-soil of the adjacent Continental Shelf—*not* over its superjacent waters. Even so, the American Government appeared to have considerable hopes of economic advantage from their claims. In his Annual Report for

1945, the Secretary of the Interior, Mr. Harold L. Ickes, wrote : 'Alaska cost us $7,200,000, the Dutch West Indies $25,000,000, the Louisiana Purchase $27,000,000. The Continental Shelf cost only the forethought that was required to assert our sovereignty over it.' (3)

Between 1945 and 1950, twelve other American Republics made similar claims, Argentina going so far as to proclaim full sovereign rights over the epi-continental sea as well as the Continental Shelf. This is an interesting case because Argentina bases its claims to the Falkland Islands on the argument that they constitute an extension of the Argentinian part of the South American Shelf.

To sum up then, since 1945, nineteen States including the United Kingdom (its colonies and protectorates as one) have adopted the attitude that a coastal State has rights and jurisdiction over the resources of the Continental Shelf adjacent to its coast. The exploitation of such resources has nowhere reached large-scale production yet, but economic demands and scientific progress in both geological and biological knowledge, together with potential expansion of technical processes may lead to international consequences which could have serious repercussions in world affairs.

The proposals of the International Law Commission include a recommendation that sovereign rights over the Continental Shelf should be recognised '. . . only for the purpose of exploring and exploiting its natural resources.' So far, petroleum is the only important 'natural resource' which has been exploited. Decreasing supplies from existing fields of this valuable material have already led to considerable experimental work in the Gulf of Mexico. The maximum distance from the coast for working oil (as far as the present writer has been able to discover) is 25 miles in the Gulf where the Magnolia Petroleum Company has discovered oil. Sixteen wells were drilled at a cost of $26,000,000 over a period of three years but only one came into production. The hazards and costs of drilling under water are very great and may only be justified if supplies from land fields fall off rapidly.

Up to the present, complicated drilling apparatus has been installed and used on floating pontoons or converted vessels. One authority has suggested that galleries and pipe lines should be driven through the sea floor from the land. This would avoid the legal problems associated with the superjacent waters over oil pools. Floating equipment and constructions cannot be legally considered as islands because of their temporary character but they may create hindrances to the free use of the sea. It has been suggested that each of these erections should be enclosed by a narrow security zone demarcated by a line of buoys. Such an arrangement would reduce obstacles to navigation and dangers from fire but would not eliminate the risk of pollution of the waters which might well interfere with fishing.

There may have been exaggeration of the potential wealth of the Continental Shelves in mineral resources, but there can be no doubt about the rapidity of the consumption of minerals in this industrial age and there seems little likelihood of any diminution in demand. In turn, the search for new supplies must continue and probably with increasing urgency. It does not necessarily follow that, because the methods of exploiting the sub-soil have not yet been very successful in terms of amounts produced, the quest for minerals will cease. The cost will inevitably be high but, in view of the continued tension in world affairs with which are associated large-scale armament plans, we should not be surprised if even greater efforts are made to exploit the Continental Shelves.

The present position is hardly more than one of claim staking. A twentieth-century Alexander could say with justice that there are no more worlds to conquer, at least on *terra firma*. The land surface of the earth has already been shared out ; Imperialism in the nineteenth century sense has ceased. Now man's attention has been turned to the sea floor and its sub-soil and to the possibilities of their exploitation together with the concomitant territorial problems. That is why the various claims to jurisdiction over the Shelf are important and why the International Law Commission has worked so strenuously to find an acceptable formula for the delimitation of areas over which

littoral States should exercise sovereign rights. Changing conditions, in terms of strategy and economics, have evoked new methods of boundary making. This is but a part of the process which is usually known as adaptation to environmental conditions and, if the evidence from the past can give us any guidance, there will be numerous boundary disputes in the future.

The problems associated with the Territorial Sea may be regarded as aspects of the wider Continental Shelf problems. Until recently, national jurisdiction ceased at the seaward limit of the Territorial Sea. For our present purposes, it is irrelevant that international lawyers could not agree as to whether the legal status of the High Seas was better described as *res nullius* or as *res communis*. For all practical purposes, the High Seas were free to all their users and, in peacetime no State, littoral or otherwise, possessed authority to interfere on them. On the other hand, the coastal State possessed full sovereign rights over the whole of the Territorial Sea including its waters, the air space over them and the sea floor and its sub-soil beneath them. In effect then, the Territorial Sea and its associated air and submarine spaces were regarded as an extension of the State territory. The outer boundary of this sea was normally taken at the three-mile limit, i.e., at three nautical miles from the low water line. The only modification of sovereign rights allowed within that limit was that of 'innocent passage.'

In recent years, two factors have been primarily responsible for numerous demands for extensions of the limits of Territorial Seas. First, the old cannon-shot rule is clearly obsolete. Indeed, it is debatable whether any width of such adjacent zones would be effective in wartime but obsession with security measures has given rise to some extravagant claims.

Secondly, the appalling disproportion between world food supplies and rapidly increasing populations has directed greater attention to the value of fishery resources. Coastal States have absolute rights over the fisheries of their Territorial Seas and it is to their economic advantage to extend this area of control. At the same time, such extensions amount to reductions of the rights of other fishing interests and may lead to serious conflicts such as that produced by the Icelandic Act of March 1952. While it is true that the Anglo-Norwegian Fisheries Dispute was settled amicably by the Court of International Justice in December 1952, other disputes may not be brought to similar conclusions, especially as there is such a wide range of claims.

At the present time, there is no universal agreement on the width of Territorial Seas. Thirty States (including the United Kingdom and the French Union) claim a three-mile limit, four States claim a four-mile limit, one State (Honduras) claims a five-mile limit, nine States claim a six-mile limit, one State (Mexico) claims a nine-mile limit and one State (U.S.S.R.), claims a twelve-mile limit. These forty-six countries include all the important coastal States, except China which so far has made no specific claim. It is significant that the States which appear to be satisfied with the three-mile limit are those which are most actively engaged in world maritime trade, in fact they own some 80 per cent. of the world's registered merchant shipping tonnage. In other words, the large majority of coastal States recognise that extensions of the Territorial Seas mean infringements of the freedom of usage of the High Seas. In view of the fact that the greatest part of the world's trade is sea-borne and is essential to the welfare of the world's population, it seems that claims to territorial expansion in this way should not be allowed in international law. Freedom of the High Seas for navigational purposes must surely be maintained in the interests of the society of nations.

In the case of fisheries outside the Territorial Seas, international action provides the best remedy for over-fishing and the best means of conserving stocks. Already in May 1952, an International Convention for the High Seas Fisheries of the North Pacific Ocean was signed by the U.S.A., Canada and Japan. The three signatories agreed to abstain from further exploitation of specified fish stocks which are already exploited to the maximum, and to carry out conservation measures. While it is admitted that there are, as yet,

no means of compelling the parties to such agreements to carry out their undertakings, short of war, this method of dealing with the usage of the High Seas is infinitely preferable to that of territorial expansion by unilateral action. Otherwise, there can be no generally acceptable means of delimiting the seaward extensions of national territories and the present well-nigh chaotic state will continue.

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NEW ADVANCES IN UNDERWATER OBSERVATIONS

At the Association's Annual Meeting in Liverpool, 1953, Sections A (Physics), C (Geology), and D (Zoology) devoted the morning of Monday, September 7 to a joint discussion of some of the newer methods of making underwater observations. Dr. H. G. Vevers, The Marine Biological Association, Plymouth, gave an account of his work on grounds near Plymouth with a remotely controlled underwater camera. Mr. R. E. Craig, The Marine Laboratory, Aberdeen, dealt with the use of the echo sounder in marine biological and commercial fisheries work. Mr. W. D. Chesterman, Royal Naval Scientific Service, discussed the use of frogmen equipped with cameras and showed two excellent films made under his direction, one in monochrome and one in colour. Dr. H. Barnes, The Scottish Marine Biological Association, Millport, gave an account of development work in the use of underwater television in marine biology. Dr. T. F. Gaskell, Anglo-Iranian Oil Company, discussed the seismic measurements made in the Pacific during a recent expedition of H.M.S. *Challenger*.

Dr. H. Barnes has edited the papers which appear below in the order in which they were given.

Photography of the Sea Floor

by

Dr. H. G. Vevers

Any evaluation of the resources of the sea is only possible if we know something of the number of animals and plants living there. Counts or estimates must be made, not only of the marketable fishes and shellfish, but also of other organisms on which fish are feeding or with which the fish are competing for food. Animals and plants of the plankton can be sampled by the established technique of townetting with standard fine-mesh silk nets, but the animals of the bottom are more difficult to sample quantitatively. Many of these animals are important as food for the bottom-feeding fish such as cod. The numbers of some of the smaller animals can be estimated from the hauls of special mechanical grabs, but this method cannot be used for the larger animals of the sea bottom.

In my work the essential requirement has been a large number of workable pictures taken at known positions in a limited area. It was decided that photo-flash illumination would not be suitable, since although this method would give a small number of high-quality pictures at each lowering of the camera, it could not easily be used to obtain a large number of consecutive pictures at a single lowering. It would, of course, be possible to use an electronic flash as light source, if the supply and conversion unit were mounted in a pressure-proof container and lowered with the camera, but this would make the whole apparatus very inconvenient to handle at sea. It was, therefore, decided to use a battery of photoflood lamps as a source of light for the present apparatus, and this method has proved relatively successful.

The camera is mounted near the top of a vertical metal tube and the lamps are arranged asymmetrically round a metal ring attached to the pole at a height of 0·65 m. from the bottom of the pole. A foot switch is mounted in the base of the pole. Between the foot switch and the attachment of the lamp ring to the pole there is a gun-metal junction box for the electrical leads.

The camera itself is a Robot II fitted with a Zeiss Tessar 3·75 cm. lens. It is housed in a gun-metal case fitted with half-inch plate-glass window. The camera switch is depressed by a solenoid mounted behind the camera and activated by the foot switch, which consists of a 21 cm.

diameter brass plate connected by a socket joint to a small sealed metal bottle containing a mercury switch. When the foot touches the bottom it is driven upwards and inverts the mercury switch which activates the solenoid in the camera case.

Power from the ship's mains (200–210 v.) is supplied to the junction box near the base of the pole by a 7-core rubber-covered cable. From the junction box four leads go to the lamps and two connect the foot switch and the camera solenoid. On deck the power is lead through a control box before entering the main electric cable. This box is fitted with fuses for the camera and lamp circuits and for the ship's power supply, as well as with switches and ammeters for the camera and light circuits. It also has a counter which records the number of pictures taken and a buzzer which rings after each picture has been taken.

In use the ship is stopped and the depth of water noted. The apparatus is then lowered on a wire warp led over the aft boom to the main winch. When the camera is 3–5 m. from the bottom the speed of lowering is reduced, the lamps are switched on and the lamp ammeter in the control box gives a reading. At the same time the camera switch in the control box is switched on, and as soon as the foot touches the bottom the camera circuit is completed, and the first picture is taken, the film being then automatically wound on. When this happens the camera ammeter in the control box gives a reading and at the same time the counter clicks on to record the number of the photograph, whilst the buzzer rings to warn the crew that the bottom has been reached. The winch is immediately stopped and the apparatus is then lifted from 1 to 2 m. off the bottom by hand hauling on the warp.

The ship is then allowed to drift for a known interval of time, the camera is again lowered steadily and the next picture taken. This procedure is repeated until the whole film has been used (40–45 exposures). The time interval between each exposure can be varied according to the speed of drift of the ship, but it is usually of the order of 30 seconds, so that a film of 40 exposures can be taken in 20 minutes.

One of the main advantages of this method of operation is that it is possible to plot with reasonable accuracy the location of each photograph on a large-scale chart of the area. To do this it is only necessary to know the positions and time of the first and last pictures, and the time interval between each exposure. This method is, of course, similar in principle of line quadrant surveys which have been so extensively used in plant ecology. The area of the sea bottom photographed at each exposure is either 1 sq. metre or a quarter sq. metre, the focus of the camera being preset.

This method of photographing the sea bottom has been used successfully on a number of different grounds in the Plymouth area in depths down to 35–40 fathoms (which is rather beyond the normal range of a diver) and thousands of pictures of the sea bottom have been taken. As the number of exposures made at each lowering of the camera is relatively large it is considered that this photographic method gives a good estimate of the density of epifaunal invertebrates.

The most satisfactory method of interpreting the photographs and counting the larger invertebrates was by examining the negatives under a binocular dissecting microscope. When there are large concentrations of animals on a single frame counting was facilitated by the use of some form of grid, and in conjunction with a micrometer slide it is possible to obtain size measurements of the animals counted.

The preliminary results from photographs of four different areas suggest that the method can be used as a competent and reasonably accurate tool, not only for the estimation of epifaunal invertebrates, which is its primary purpose, but also for counts of those infauna species which produce a tube or burrow with an opening on the surface of the sea-bed. Counts of these burrows would help to confirm estimates made from the catches of bottom-sampling gear. Before this can be done, however, more information must be obtained on the shape, size and general appearance of these surface holes, so that the species producing them can be easily distinguished. The results so far obtained have given much interesting information on the way in which certain animals are distributed on the sea-bed. Crabs, hermit crabs, bryozoans, starfishes, molluscs and

a few fish have all appeared on the photographs at various times, but the most interesting pictures have been those of some very dense populations of the common brittle-star. We have found that this animal, which has an arm span of about 4–5 inches, can occur at a density of over 100 individuals per sq. metre, in beds which may extend over an area of 4–6 miles long and 2 miles wide. The brittle-stars feed on organic matter in suspension as well as on deposited detritus, and the to and fro action of the offshore tidal streams probably brings them a rich and fairly continuous supply of suitable food in suspension. Indeed, in one restricted area, where the bottom was rocky, there were some 340 brittle-stars to the sq. metre.

The number of photographs required to give an adequate estimate of the density of animals on a ground will clearly depend upon the nature of the bottom deposit and the type and habits of the species living there. On a poor ground, or on a ground with heterogeneous bottom deposits, such as the muddy sand and gravel area south-east of Looe Island, a relatively large number of pictures, perhaps covering 300–500 m.^{2} for 2–4 square miles in area, would be required in order to obtain a reasonably good estimate of animal density. In the latter ground there is considerable variation in the numbers and types of animals found along each of the three transects photographed.

Echo-sounding in Marine Biology

by

Mr. R. E. Craig

We shall first describe the action of the instrument, which was introduced as an aid to navigation, the principle involved depending on timing the interval between a sound transmission and its echo from the sea floor. In modern sounders the transmission which lasts for about one thousandth of a second is a high frequency pulse, commonly about 15 kilocycles, i.e. just beyond the audible limit. In Figure 1 is shown one of the methods used to give a pulse of this kind. Passage of a current through the winding produces a magnetic field round the nickel discs, which causes them to contract as a result of the magneto-striction effect. If the winding is made part of a tuned circuit, the stack of discs will then resonate at its natural frequency. The resulting sound wave goes out horizontally, but is deflected downwards by the double walled reflector, whose effectiveness is due to the air which is a good reflector of underwater sound. To get the necessary short pulse a condenser C is discharged through the oscillator windings by closing the switch.

If the nickel rings are previously magnetized, such an oscillator can operate as a receiver. Sound waves falling on the stack of rings makes them vibrate, and set up a voltage in the windings which can be amplified and detected. The speed of sound in water is near enough 1,500 metres per second (though where special accuracy is required this must be corrected for changes in the temperature and salinity of the water) so that a time of one second between the transmission and reception of the echo corresponds to a depth of 750 metres.

One of the most common and satisfactory methods of presenting the information is by means of paper recording (Figure 2). A stylus is carried on the end of an arm which rotates at, for example, 60 r.p.m. During a part of its motion, the stylus brushes over a strip of recording paper, and rollers pull this paper along at a steady slow speed, say a quarter-inch per minute. It is arranged for the transmission to occur as the stylus passes the point X, and for the returning echo to give an electrical impulse on the stylus. This impulse makes a mark on the paper, and it will be clear that the deeper the water, the further down the paper will this mark appear, and that if the ship is moving the result is a tracing of the form of the sea-bed, for the successive marks build up into an almost continuous line. The usual recording paper is impregnated with starch solution and potassium iodide, and is used moist. Passage of a current through this paper leaves free iodine which combines with the starch to make a distinct brown mark. Dry recording paper is being gradually introduced, while for some applications a cathode ray tube presentation may be preferred.

Figure 3 is a reproduction of a typical recording. The heavy line is produced by

the transmission, so its upper edge corresponds to zero depth. Below it is the echo from the sea-bed, and below this again we may find a second or third echo.

Before passing on to the use of echo-sounding in fishery work I would like to emphasise that boats with echo sounders are to be found in almost every fishing port, and that it is, therefore, a simple and cheap matter to obtain useful information in depths as shallow as four feet below the boat's keel—information about the lie of rock formations, gullies or banks, that could save labour or give increased confidence in the interpretation of beach or coastal geology. The discovery of deep ravines or trenches in the edge of the continental shelf is one of the major contributions of echo-sounding to geology, and the explanation of these is a contemporary problem of great interest. I would also like to emphasise that typical recordings, for example, of the uncanny flatness of great areas of the sea bed or of the rugged floor of the Minch, ought to be seen by every student of geology. There is something so real and compelling about such records that they deserve a place in the ' sub-conscious ' of everyone who aspires to say how this earth has been fashioned.

Echo-sounding in fishery work begins with the observations of Mr. Ronald Balls, of Great Yarmouth, in 1933, who noticed that he sometimes obtained echoes at a depth less than that of the bottom. After discussion with Dr. W. Hodgson of the Ministry of Agriculture and Fisheries Laboratory at Lowestoft, he came to the conclusion that these might be due to shoals of fish, probably herring. He made a lengthy and careful investigation, and showed that by shooting his nets where these ' ghost ' echoes appeared, he caught, on an average, more herring than other drifters. Thus he was able to show, at one and the same time, both the correctness of his theory and its practical value. The echo-sounder he was using was not of the recording pattern, but of an earlier type, with which the individual echoes had to be noted at the time, and requiring, therefore, continual attention. Almost at the same time, the distinguished Norwegian scientist, Oscar Sund, made a similar discovery for cod using a new recording machine. Two years earlier than this, the hydrographer of the Royal Navy had observed fish echoes, but these were reported in a journal of limited circulation, and failed to arouse interest. It seems clear that it was the work of Skipper Balls which began the revolution in fishing technique, for his findings were brought to the notice of the manufacturers who publicised it through their advertising. In Figure 4 is an example of a fish echo. This is from a shoal of herring, containing perhaps fifty thousand fish.

From the point of view of scientific research we have, in the echo-sounder, a new tool which both demands and influences research. First, we wish to identify the recorded echoes. Once this is done, we can study the behaviour and distribution of animals in a way previously impossible. Then we want to understand the mode of operation, so that new machines can be as effective as possible. The problem of identification has been studied in most countries where fishing is important, but no rules of widespread application have been devised to establish specific identity. However, we can tell a good deal about the type of shoal. Particular individual traces have often been identified, of course, and it depends entirely upon local knowledge whether it is possible to say that in any given area *all* traces of a particular type have the same cause. A great deal of work, which has begun with the aim of identifying echo traces, has ended by increasing our knowledge of the fish population rather than giving a general answer to the problem, and a process of elimination is often required, calling on all existing knowledge. For sampling ' on the spot,' fishing lines and ring nets have been of service. Where planktonic organisms are suspected to be important, suitable silk nets have to be used.

To identify the organisms recorded on the sounder, workers both in Norway and in this country have tried to use cameras, and for this purpose it is preferable to use flash illumination, since any fixed light may change the behaviour of the fish. In fact even the lowering of the camera may sometimes modify the echo trace. Where the fish are not deep, it has been found that small explosive charges are very useful ; they kill the fish which then come up to the surface.

The second requirement is to study the way in which echoes are produced. If the sensitivity of the machine and the polar diagram of its oscillators are known, it may be predicted how any arrangement of targets will appear. This depends on knowing also the physical constants of the targets, which will usually be fish. Ideally, the scattering cross section is required (i.e. a measure of the size of the echo to be expected) for each size of each species of fish—and not only fish, but also planktonic organisms—and this information is required for a wide range of frequencies if we are ever to give a truly scientific account of the subject. It would also be of interest to know whether this cross section varies when organisms migrate vertically. A fish with a gas bladder for instance, might have a smaller cross section when descending than when it is coming up, but we do not know how much smaller.

The most important contribution of echo sounding to fisheries biology has been the possibility of making rapid surveys of pelagic fish. A single research ship can plot out in the course of a season the changes in distribution of the fish, and in successive years get a comparison of the populations. It will be understood that this refers to pelagic fish—in our waters, herring, pilchards, mackerel, sprat—while in certain overseas fisheries cod, for example, may be usefully sought. Other fish show up, if they happen to be distinctly above the sea-bed, but it is only the truly pelagic fish that can be surveyed in any significant way at present. In Figure 5 are shown some examples of surveys in which echo traces have been plotted out and compared with catches. A reasonable measure of agreement will be noted.

A comparison of what can be done by a single ship with what she could achieve by other methods is striking. Results of this kind, besides their use to the fisheries scientist, are of value in making a fishery more productive, and this use is now recognised and will probably become a regular feature of pelagic fishing. In addition to giving the area distribution of fish, surveys indicate what sort of vertical migrations are taking place, and what changes in shoaling behaviour ; and the extra information which can be obtained gives added stimulus to hydrographic and plankton studies.

In addition the echo-sounder makes it possible to study the effects of artificial stimuli in a natural background. The effects of sound, or of artificial lights, can be studied in this way, provided we can be sure what we are stimulating. In Figure 6 is an example of such an experiment at night. The thin heavy line is an echo from the lamp (the ship was drifting slowly at the time) and this lamp, which had a 125 watt ultra-violet bulb, was switched on at point A. At B it was raised, and at C lowered sharply. It is clear that marked effects are caused by the light, and similar effects can be obtained using a normal tungsten lamp. The heavy trace in the recording was at the time assumed to be plankton, because of its slow vertical movement. It has however been possible to repeat these results and to take advantage of the conditions which are ideal for camera work. In a trace of similar form to that illustrated fifteen exposures out of twenty were blank, and five showed only whiting.

A most important example of the contribution made to oceanography by echo sounding is provided by the work on the Deep Scattering Layer. This has been detected in the deep water of all the oceans, being only observed with sounders having more than the usual transmission energy. It appears as a somewhat variable band on the recording paper—sometimes a single band, sometimes more than one. This layer appears at a depth of 300 to 400 metres by day and rises at nightfall close to the surface, generally becoming so diffuse as to be lost to the recording. At dawn the layer or layers are seen to reform and descend. These layers may go down at about twelve feet per minute, and rise rather faster.

Because of this habit of migration, it is considered that the deep scattering layer is produced by living things, and many suggestions have been made regarding their identity. American opinion seems to favour the idea that euphausids—oceanic shrimps ranging up to two inches in length —may be the cause. Alternatively, bathypelagic fish, which range up to about four inches for the commoner ones, and squids or other predators feeding on euphausids,

have been suggested. Most of the bathypelagic fish have swim bladders, and so would be expected to give strong echoes in relation to their numbers. However, we do not yet have complete information about the scattering layer, for we would like to know the seasonal variations everywhere. We may say that the problem awaits a firm answer, but it may be of interest to note that the speeds of ascent and descent (descent being more rapid) have been closely matched by a trace thought to be fish, under the influence of artificial light.

We have indicated a few ways in which echo-sounding is helping to answer some biological problems, and seen the sort of new questions it is posing. However, in our herring fisheries the echo-sounder is no longer a novelty. Of the 500 boats which take part in the Scottish herring fishing, at least three-quarters are already fitted with echo-sounders, and it is likely that within a year or so, all will be. Herring catching has always been more of a gamble than other types of fishing, and this is because of the mobility of herring and their patchy distribution, and also because they have to be met with the net at the right depth as well as in the right area. With the use of the echo-sounder the job has become more continuous and more reliable, because we now try to meet the shoals wherever they may be rather than depending only on traditional grounds which have been at times rich, but have yet been fickle.

One of the most striking successes is due to ASDIC rather than conventional echo-sounding. ASDIC was developed for the detection of submarines, and works on the same principle as echo-sounding, but the beam searches out horizontally instead of downward, and the equipment is more powerful and of greater range. Using ASDIC, the *G. O. Sars*, the Norwegian research vessel, has introduced their fishing fleets to an oceanic fishery for fat herring in the Norwegian Sea, and between Iceland and Faroēs. Dr. Finn Devold, writing of this work, refers to the importance of finding the routes taken by shoals coming into the Norwegian coast and says, 'In some years about 20,000 fishermen might be idle for a whole month awaiting the arrival of the herring in coastal waters.' Describing the results of shadowing the shoals in the winter of 1951 to 1952, he reports : 'In a day and a half about 40,000 tons of herring were caught, having a sale value considerably above what it had cost to build and equip the *G. O. Sars*.'

In the story of the herring fisheries, echo methods should be seen as a revolution comparable with the introduction of steam, and a swifter one. We see as yet only the beginning of this revolution, the search for fishing grounds by echo. A spirit of experiment in nets and gear has been stimulated by our new knowledge of fish behaviour, and particular interest centres at present on the development of various types of pelagic trawl.

The Taking of Underwater Films by Frogmen

by

Mr. W. Deryck Chesterman

I hope to demonstrate the use of underwater films by showing a number in both monochrome and colour that have been taken by the Royal Naval Scientific Service. The earliest films were shown to the Royal Society in 1950 and since then there have been considerable technical improvements in the Admiralty work. Apart from these examples we are now familiar with the excellent quality of this type of work from the recent publication of books by Cousteau and Haas. It is little wonder that underwater film work—particularly in the warmer seas—is becoming quite popular as a sport, quite apart from its scientific importance as an aid to discovery.

It is first necessary to point out that the normal helmet diver is not suitably equipped for underwater film work. His air line to the surface restricts both his movements and his range of activities—and any movement stirs up mud thereby reducing picture quality. Self-contained frogmen have therefore been employed. The camera-man breathes from compressed air bottles strapped to his back—and the restriction to depths less than 30 feet by oxygen breathing is therefore overcome. The pressure is adjusted by a demand valve equalised to ambient pressure.

Descents to 150 feet are thus possible, provided adequate decompression times are allowed in ascending to the surface. In this way the camera-man is free to swim about whilst taking his pictures : he can stop at any point of particular interest or can follow moving creatures at will.

In the early work still shots were taken, but this method has been superseded by cine cameras. Particularly for moving objects the latter are far superior, although for a detailed examination of a stationary subject a series of ' stills ' can sometimes be a useful adjunct to the cine film. A normal 35 mm. instrument is used contained in a special water-tight housing. The aperture and focus controls are brought outside the casing so these can be set and altered during working by the frogman. The camera motors are driven electrically, the power being supplied by batteries in the case and the start-stop control is brought outside to be under the control of the frogman.

The major physical problems concern illumination—whether the pictures are taken by daylight or artificial illumination. In many conditions it is not possible to work by daylight and a range of underwater lighting units has therefore been developed for harbour work and for conditions of poor water clarity. Visibility in British coastal waters may be severely restricted by suspended particulate matter —and when the amount of the latter becomes large it is quite impossible to obtain photographs of adequate quality.

There may be a great future for colour film in this work—colour film has already been exposed experimentally at depths as great as 150 feet with the aid of artificial light. However, present daylight colour films show a certain lack of colour balance since the correct filter systems have not yet been developed. More research is needed.

I can best illustrate the possibilities of the method by a number of films—which clearly illustrate the strength and limitations of the technique. In these films the general aspect of underwater life is shown and its specific use in elucidating the behaviour of underwater gear illustrated by a film of a trawl net in action. Fish can be seen fleeing in front of the net, eventually passing backward into the net—and in some cases with the smaller fish escaping from the cod end. For naval training operations the underwater film has been used to demonstrate the correct submarine escape procedures—the films for demonstration being made under actual conditions of a training operation. Films have also been made of wrecks, and these when studied at leisure have been found to be of considerable use in planning salvage work. The Royal Naval Scientific Service are also tackling the problems associated with slow motion films underwater. To demonstrate this aspect of the work I will show a film of propellers, taken on ships underway. Here, of course, the worker is guided by the long experience which has been obtained in similar studies on models carried out in the laboratory, the same or similar techniques being applied. Studies have also been made of the trajectory of underwater weapons—the ejection of submarine torpedoes, for example.

It is clear that the technique is a powerful tool in underwater investigation for both fundamental exploratory observations, and the behaviour of various underwater processes.

Underwater Television and Marine Research

by

Dr. H. Barnes

Underwater television is a natural technical development of underwater photography. As with the latter technique, this work is particularly aimed at obtaining a more detailed knowledge of animals in their natural habitat. It is a new ecological tool.

Underwater television work, as with all the photographic techniques, requires reasonably clear water for when the suspended matter in the water becomes excessive the quality of the pictures deteriorates rapidly. This development work is being carried out in the Clyde and the fact that at most seasons of the year the water is reasonably clear is a great advantage. Only after heavy winter rains and storms has picture quality been really poor.

The method is, in principle, simple : a television camera is enclosed in a watertight case and connected by a 32-core

cable to the appropriate units. Lights are provided to illuminate the scene. The ' visual ' picture collected by the television tube is brought up in the form of electrical signals and then reconstituted in terms of light by display on a cathode ray tube picture monitor. There is, of course, no radio link. The Millport apparatus employs a C.P.S. Emitron camera tube. Image orthicon tubes have been used in other underwater television units—particularly those employed by the Royal Navy. There is no perfect television pick-up tube and the most suitable tube should be chosen in relation to the type of work to be done. The image orthicon system is very sensitive and particularly useful therefore for searching, but for marine biological and associated work the C.P.S. Emitron, with its excellent picture quality, offers some advantages.

The equipment is worked from the 75 ft. *Calanus*. The rather cumbersome gear is lowered over the port quarter on a non-spin steel cable, to which the lighting and camera cables are lashed at intervals. The monitoring and control equipment is housed in the below deck laboratory which is in telephonic communication with the deck. There is 600 ft. of cable available and the gear has been worked to over 500 ft.—but at this depth the bottom of the Clyde is largely mud with a restricted epifauna and the pictures from the biological point of view are therefore often somewhat lacking in interest. In development work it is essential to have something to observe and most of the work up to date has therefore been done in the shallower water between 60–200 ft.

What are the virtues of this method? It is no exaggeration to say that it is the most exciting of these new techniques. To be able to sit and watch the behaviour of animals in their natural habitat when they are a hundred feet below the surface *is* exciting. It has all the fascination of the cinema over still pictures and all the interest associated with watching events whilst they are actually happening. However, we must examine critically the value of this new technique in marine science—both in relation to the other newer techniques which have been considered in this symposium as well as to the older methods.

Let us take the disadvantages. First, the biological material is only seen, not brought up for examination. Underwater television and, indeed, all the other methods which have been discussed in this symposium can never therefore replace the classical techniques of marine zoology for detailed morphological, physiological and biochemical investigations. This is an important point to realise, since popular interest in these newer techniques and particularly in television, has often led to rather uncritical and inaccurate assessment of their place in underwater research. These newer methods may be useful accessories in searching out where material may be found and most easily collected by conventional methods.

Secondly, the capital outlay is high and running costs are comparatively heavy. This is perhaps not of great importance in a new technique with a wide range of future applications, but as underwater television becomes more generally used this factor must be taken into account. Whilst many marine biologists can, and indeed do, become expert photographers (although it must be pointed out that a much smaller number become expert divers) the average marine biologist can hardly be expected to service the complicated electronics of a television camera and receiver. A qualified technician is, therefore, necessary. Development projects are well known to be expensive and it may be confidently expected in the light of experience now being gained in underwater television research that an increased demand for such equipment will lead to simplification in design and resultant economies in both capital expenditure and running costs. It must be stressed, however, that for detailed underwater work such simplifications must not proceed at the expense of picture quality, for this is a paramount desirability.

Thirdly, the restrictions imposed by turbid water and which are common to underwater photography and underwater television put these methods at a disadvantage when compared with the echo sounding techniques which have just been discussed. However, it is only fair to point out that the information obtained by the latter is of a somewhat ' lower order.'

Fourthly, the actual apparatus with which we have been working is rather

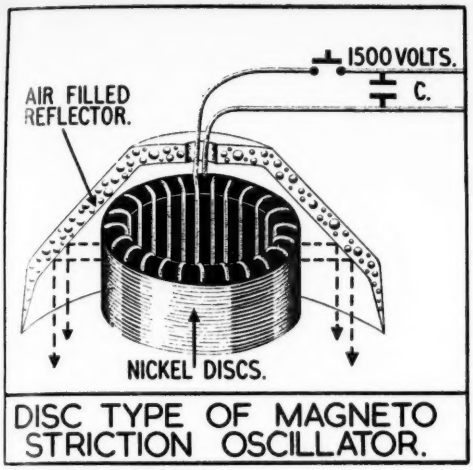

FIG. 1.—A ring-type magneto-striction oscillator (diagrammatic only).

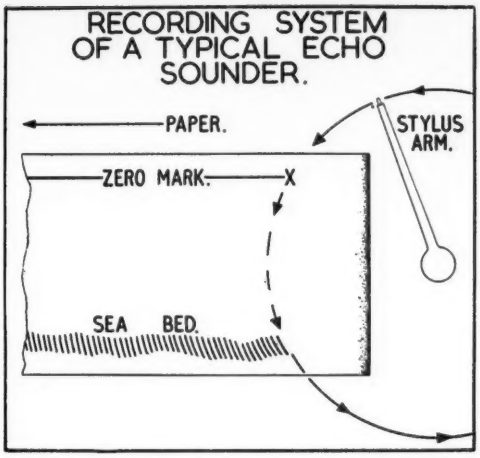

FIG. 2.—Principles of the paper recording system used in many echo-sounders.

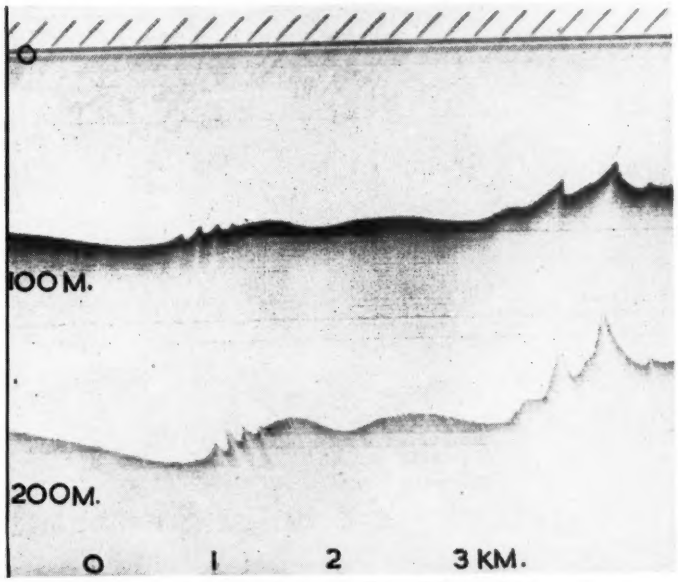

FIG. 3.—A typical recording of the sea-bed.

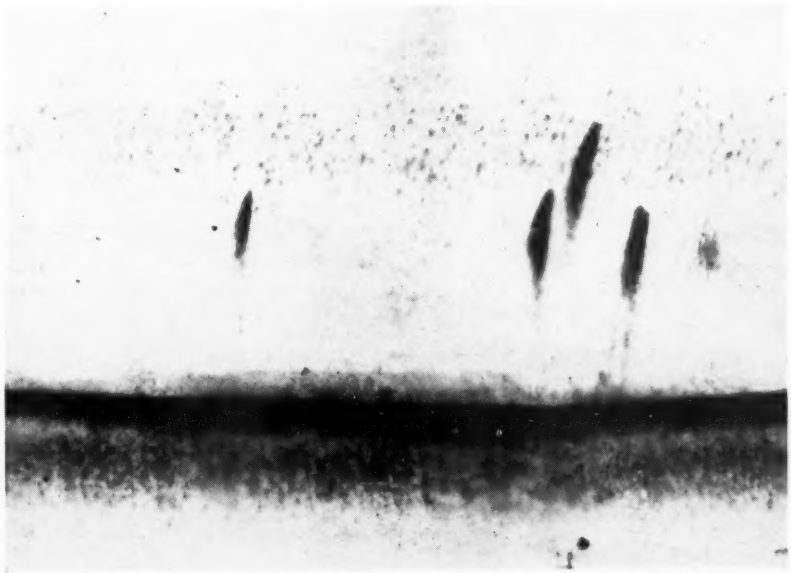

FIG. 4.—A common form of fish echo. These are shoals of herring.

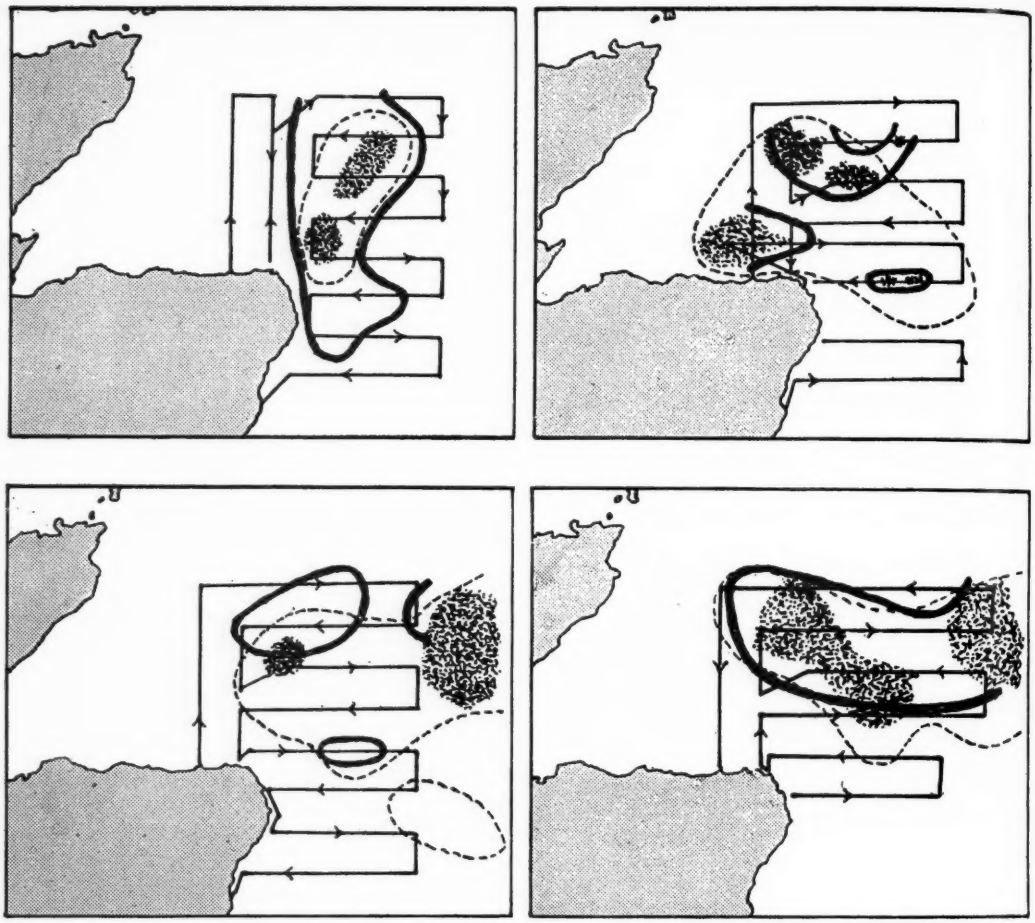

FIG. 5.

Charts showing—

Heavy lines—areas of best echo traces.
Shaded areas—areas of best catches for the week in question.
Dotted lines—limits of fishing.

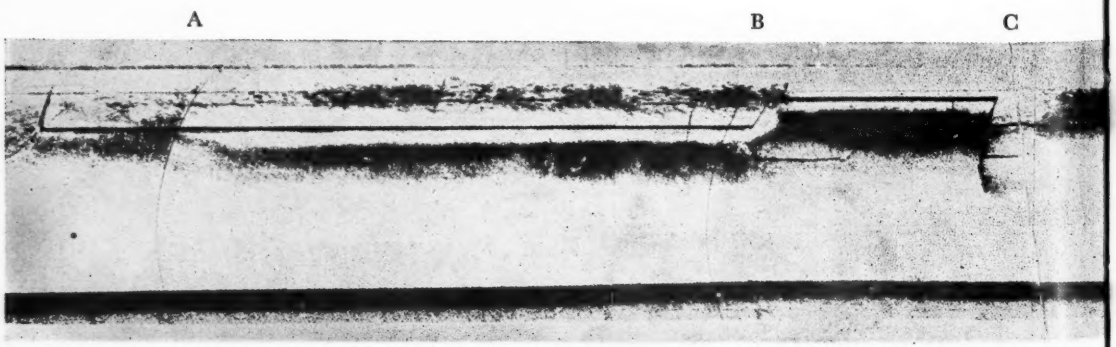

FIG. 6.—Echo trace from an experiment in The Minch in which an artificial light has been used to influence t organisms producing echoes. (See p. 53.)

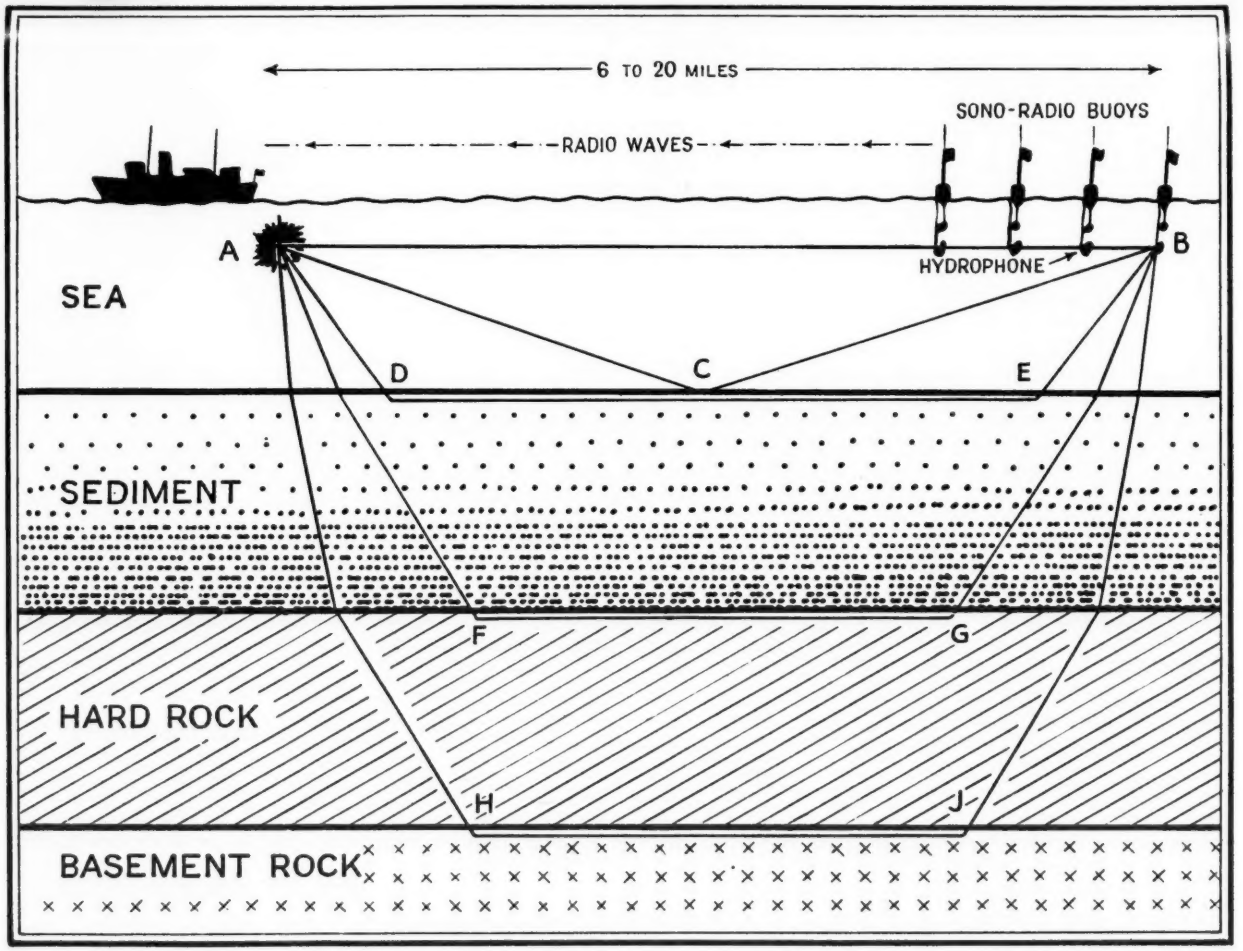

FIG. 7 (of Gaskell).—To illustrate refraction methods. Explosion is set off at A. Direct wave in water AB. Reflected waves from sea-bed ACB. Refracted waves, ADEB, AFGB and AHJB. All waves are picked up by hydrophones on sono-radio buoys and returned via radio link to the ship.

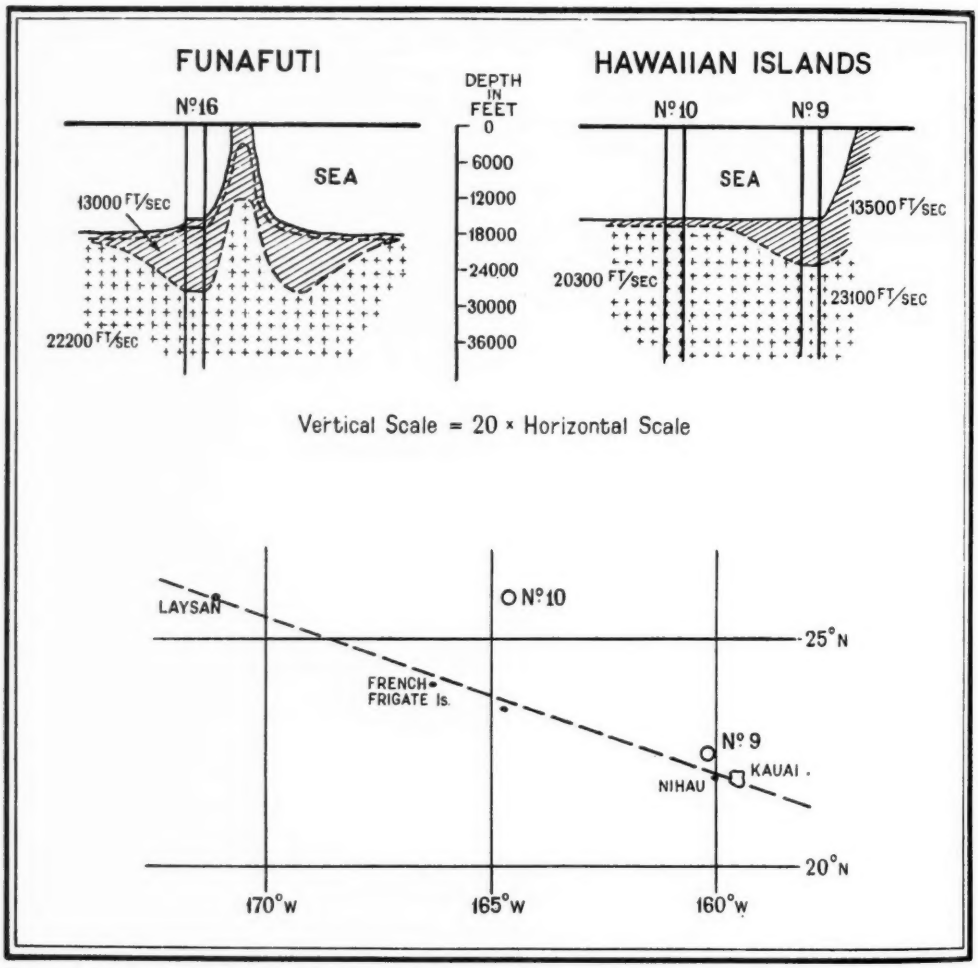

FIG. 8 (of Gaskell).—Geological sections obtained by seismic refraction methods at Funafuti atoll (Ellice Islands) and the Hawaiian Islands. The chart shows the location of the Hawaiian island stations worked with the methods described. See text for explanation of the upper figures.

cumbersome and, although even with further development the *best* pictures will still probably require somewhat cumbersome apparatus and reasonable weather conditions for working, simpler and more easily managed gear could be, and indeed has been, designed.

Fifthly, when artificial light is employed its effect on the behaviour of the organisms must be taken into account; this is a problem common to many experimental techniques. It should not be allowed to discourage experimental work, particularly in the early stages of development, but it is a factor which must be constantly borne in mind when assessing the results.

Perhaps we can now look at the other side of the picture—the advantages of this technique. First, compared with the older techniques it can, like underwater photography, give detailed information on the distribution of the bottom fauna and the relations of one set of organisms to another and to the physical environment. The methods of capture normally employed, whilst capable of giving general answers to distribution problems, can never give detailed ecological information.

Secondly, it has the enormous advantage over underwater photography that the scene can be viewed continuously and records made at the time by the biologist himself. The viewing can, at any rate within the limits of human endurance and the financial and technical resources of maintenance, be virtually continuous.

Also since the scene is continuously under view adjustments can be made by the remote control mechanisms so that the best picture can be produced of a given scene during viewing—a feature not present in underwater photography—and one which tends to better picture quality, and hence more detailed biological information.

Thirdly, when compared with diving techniques the continuity of observation is again an important feature and, further, the fact that the biologist himself is able to observe in this way rather than merely to inspect a record taken by a professional diver is a great advantage. To this must be added the facts that the depth of working is virtually unlimited and that working at great depths does not involve risk to personnel.

It is, of course, evident for a major attack on some problems a real *tour de force* could be accomplished by the application of all techniques at the one time and place.

And now we must turn to the results. We have had many hours' viewing and it is neither necessary nor desirable to describe in detail the many things which have already been seen. Rather we must attempt to assess the potentialities. The instrument is undoubtedly of importance for the study of the epifauna both biologically and in its relation to the physical environment. If the species present in an area are known it is not difficult, after practice, to distinguish between them—and if in doubt animals can be lowered with the camera for practice recognition. Dense colonies of Ophiuroids, *Luidia ciliaris* preying on them and seeming to sweep whole areas clean have been observed. The behaviour of animals can be watched—the fighting of crabs, the behaviour of bottom living creatures in relation to their burrows—all come within this category. Physical features of the bottom—aggregation of shells of fixed orientation, sand ripples, traces left by animals dragging over mud surfaces have all been investigated.

All these are stationary or sluggish subjects. With fast moving animals such as fish the technical difficulties become greater. When close up and easily recognisable they are quickly out of the field of view and when far away they are recognised only with difficulty.

So far the work has been largely exploratory: there is no doubt that the technique is a powerful one but much remains to be done. Perhaps its greatest use is in fields as yet untouched.

Seismic Measurements in the Pacific

by

Dr. T. F. Gaskell

The object of the techniques so far described has been to give a more accurate picture of the relation of animals to their environment and to one another. Mention of the application to applied science has been made—the echo-sounder to commercial fisheries and underwater cameras to problems of naval and salvage interest.

I have to deal with the use of sound, not to learn of the disposition of animals and things, but to understand something about the structure of the ocean floor itself. Since the ocean depths are usually covered with one or two thousand feet of sediment which is derived from atmospheric dust and matter from the sea, bottom samples obtained by the conventional methods are of little help, although it is occasionally practicable to obtain samples from undersea rock outcrops in deep water. By the use of sonic methods, however, it is now feasible to study geological formations several miles below the sea-bed surface, and from the results obtained to provide answers to long-standing questions such as the permanence of ocean basins. The methods are those of the geologist and geophysicist and are analogous to those used in geological land surveys and prospecting. The results to be described were obtained during the recent world-wide expedition of H.M.S. *Challenger*, when measurements were made to elucidate the submarine geology of the Pacific. The most interesting results from a zoological point of view were probably those relating to the structure of isolated islands and coral atolls, since the behaviour of these habitats of marine life in the past and their present development must have some bearing on biological theories. H.M.S. *Challenger* was equipped for deep ocean sounding and added a great deal, including the world's deepest sounding, to our knowledge of sea-bed topography.

In order to probe into the layers that constitute the earth's crust, sound impulses are sent out by means of discrete explosions and are recorded after they have travelled into and back from the various geological formations below the sea. The simplest method of obtaining information is to time the wave that reflects from the interface of two media, but it is necessary to know the velocity of sound in the upper layer in order to calculate the depth to the interface, and in the oceans the constitution of the various layers is not really well enough known even to make a good guess. Furthermore, the reflections always arrive after the very strong wave that travels through the water, and are generally obscured. There is, however, a wave that can arrive before any others, and its arrival time can be measured accurately. This wave is the refracted wave (ADEB in Figure 7) and it arrives before the direct sea-water wave because the fast travel in the sea-bed more than compensates for the extra path length. Refracted waves may travel in deeper layers (AFGB and AHJB in Figure 7), and the deeper the layer the longer is the horizontal distance needed to observe the refracted wave as a first arrival. By measuring arrival times at various distances the velocity of sound in each rock layer can be determined, as well as the thickness of each layer. It is necessary for the velocity in each layer to be greater than that in the one above, and generally nature is helpful in this respect. If the interface between the layers is not horizontal, information about the dip can be obtained by firing explosions from both ends of the line of recording instruments. Both reflection and refraction methods have been used extensively for many years on land, particularly in searching for oil-bearing geological formations.

The seismic refraction technique is illustrated in Figure 7. The impulses from the explosion, which is normally a depth charge, are picked up by hydrophones suspended beneath buoys, which are linked to the ship by radio. The signals received by each hydrophone are recorded by a separate galvanometer on moving photographic paper, marked with timing lines, so that the arrival time of any impulse can be accurately recorded. The distance between explosion and hydrophone is given by the travel time of the direct wave through the water, since the velocity of sound in sea water is well known. In deep water the refracted wave from the sedimentary layer (ADEB, Figure 7) seldom appears as a first arrival because it is masked by the direct water wave at short distances and by the hard rock refracted wave (AFGB) at long distances. The thickness of the sediment is indicated, however, by the arrival time of the refracted wave in the hard rock layer, and is calculated assuming a velocity of 7,000 ft./sec. for the sediment; this figure is based on observations in shallow areas where velocity measurements are possible, and on laboratory measurements which can be made on compressed cores from the sea-bed.

The various rock layers below the sediment all give refracted waves whose velocities can be measured, so that such a seismic experiment as this, gives a picture of the structural column at any one place, the different layers being labelled by their respective sound velocities. The geological identification is carried out by comparison of the velocities with measurements on known rock on land, both in the field and in the laboratory ; it is unfortunate that most rocks have a wide range of velocity, limestones, for example, extending from 8,000 to 20,000 ft./sec. so that seismic work alone cannot give a complete geological picture. It does, however, give the depths of discontinuities in the rocks, and generally there is other information available which allows the interpretation of the rock type to be fairly conclusively made from the velocity measurements.

In shallow water there need be no masking of the top sediment layer by the water waves, since the hydrophones can be placed on the sea-bed. The buoys are usually anchored since sideways drift, which is not of great importance in big expanses of ocean structure, cannot be tolerated in a lagoon where the depths of the formations change rapidly with distance. The detail of the work is much greater for most shallow water experiments and can be in fact very similar to land seismic work.

H.M.S. *Challenger's* voyage round the world was in a westerly direction, across the Atlantic to Bermuda, and through the Panama Canal to Canada, Honolulu and Japan. After a run southward to New Zealand, the homeward journey was through the Indian Ocean and the Mediterranean. Seismic measurements were made in the Atlantic, Pacific and Indian Oceans, but this account is concerned with the experiments in the deep Pacific Ocean, which will be taken as seaward of the andesite line. The major part of the Pacific is 2,800 to 3,000 fathoms deep, and in these large relatively flat areas a layer of sediment 1,000 to 2,000 feet thick rests on a rock with a sound velocity of 21,000 ft./sec. The nature of the rock is not certain, but it may be basalt, and it extends downwards for about 15,000 feet until a 27,000 ft./sec. material is reached, at what is generally called the Mohorovicic discontinuity. This simple deep ocean structure becomes altered near islands, by the interposition of a material with a sound velocity of 13,500 ft./sec. This lies between the sediment and the 21,000 ft./sec. hard rock ; the interposed material is about 9,000 feet thick near the Hawaiian islands, and the normally flat 21,000 ft./sec. rock is depressed below its normal level. A similar interposed layer occurs near the island of Funafuti, and it is possible that the section at this coral atoll and at the Hawaiian islands is similar, as is shown in Figure 8. The velocity of 13,500 ft./sec. could correspond to shales, limestones or volcanic rock, but the latter is the most probable material for reasons other than seismic ones. The Hawaiian islands are volcanic and laboratory measurements show that 13,500 ft./sec. is a reasonable velocity for Hawaiian lava flow. Shallow bores in Hawaii indicate that the islands are constructed of a series of lava flows, and the steep-sided profiles of the islands, and of the other islands and sea mounts in the Pacific Ocean, are similar to those of volcanoes occurring on land. Geological evidence indicates that the older north-west end of the Hawaiian island chain has sunk, and the detailed measurements at the atolls of Funafuti and Nukufetau show that these are formed as Darwin postulated, by the growth of shallow water coral on a slowly sinking volcanic core. The seismic measurements show that there is at least 2,000 feet of comparatively low velocity material resting on 13,000 to 15,000 ft./sec. rock. The 13,000 and 15,000 ft./sec. material is identified as volcanic, and not as coral limestone, because it deepens rapidly as the reef is approached, whereas the layer above continues out to join with the new coral growing at the outer edge of the reef. The historic bore-hole at Funafuti stopped at about 1,100 feet, still in coral rock and debris, but volcanic rock was reached at Bikini and Kwajalein at several thousand feet. Below the 13,000 to 15,000 ft./sec. material is a high velocity layer, and this most probably corresponds to the hard core of the original volcano, on which rests the cone which was formed of material altered during eruption.

This example of the Pacific islands structure shows how seismic results alone

give a picture that could fit with limestone or volcanic rock, but the interpretation may be limited by other information. Although large thicknesses of limestone can exist on land they do not occur in isolated units, with a distribution similar to the islands of the Hawaiian groups. Furthermore, in the Pacific there do not appear to be any obvious sources from which great thicknesses of sediments may be derived. The details from the borings and the geological evidence from the islands confirm that the interposed 13,500 ft./sec. material is volcanic in origin. There must, of course, be some debris round the main volcanic core and some coral growth, but these occurrences are both subsequent and subsidiary to the main volcano.

The Pacific sea floor, then, is normally composed of a flat hard rock basement and at frequent intervals volcanoes erupt to form piles of volcanic material, made lighter by the effect of gas or water. The volcano generally has a hard rock core, formed of unaltered material. The seismic results suggest that the origin of this core material is below the 21,000 ft./sec. hard rock layer, because the velocity observed beneath 13,500 ft./sec. volcanic material is always higher, at about 23,000 ft./sec., than in areas of deep ocean where no interposed 13,500 ft./sec. layer is observed; perhaps the material from below the Mohorovicic discontinuity mixes with the 21,000 ft./sec. layer, or possibly the liquid rock forms near the junction of the two rock types. The mass of volcanic material is much heavier than the sea water it displaces, and the volcano slowly sinks so that the interface between 13,500 ft./sec. material and the hard rock of the sea-bed becomes depressed below the normal hard rock level. There will be a limit to the sinking, since equilibrium will be reached when the mass of rock (less that of the water it displaces) becomes equal to the upthrust given by displacement of some dense fluid medium deep down in the earth's crust. The density differences are about 1·6 for rock and water and about 0·2 or 0·3 for the deep layers. This would normally mean that sinking of some 6–8 times the final height of the cone above sea level will occur; however, the seismic results show that the cone is supported on a root more than twice the diameter of the cone, so that the sinking need only be of the same order of magnitude as the cone height. The many flat-topped sea mounts may be volcanic islands that have sunk out of sight after their tops have been worn flat by the sea; the atolls are ones that have been preserved by the activity of the coral-building polyps that have managed to keep pace with their sinking platform. Pacific islands, then, have limited lives, and they are always in a state of change. They either grow actively and are subject to volcanic outbursts, or, after reaching maturity, slowly sink and become levelled off by the sea action, especially when their surface approaches sea level. There is not a plentiful source of sediment to fill in the ocean near the lines of volcanic islands, as occurs on the fringes of continents, and it is only because temperature conditions favourable to coral growth exist in a large part of the Pacific that so many above-surface features can exist at the present time. Other topographical features have been reported in the Pacific, and these may be long folds analogous to mountain ranges on land. However, as more and more detailed sounding is carried out it becomes increasingly apparent that sea mounts and flat areas are quite common features of the Pacific Ocean.

The new seismic techniques have already advanced research in submarine geology far beyond the old sea-bed sampling carried out by the 1876 H.M.S. *Challenger* Expedition. The old laborious sinker and rope soundings, which gave only isolated depths and samples, have now been replaced by the echo-sounders which will give continuous profiles of the sea floor. Where the *Challenger* of 1876 could show that deeps existed, her modern namesake has obtained a complete section across a six-mile-deep trench. Seismic experiments at sea are costly undertakings and have only been possible with the help and co-operation of the Royal Navy and especially that given by the Hydrographer of the Navy.

SEDIMENTARY ROCKS AND TIDAL FLAT DEPOSITS

SECTION C (Geology) devoted *Session B* on the morning of Friday, September 4, 1953, to a series of papers on sedimentation. Two of these are printed below in an abbreviated form. Prof.Dr.Ph. H. Kuenen, University of Groningen, Netherlands, dealt with recent deposits and the interpretation of sedimentary rocks. Dr. L. M. J. U. van Straaten, University of Groningen, discussed the composition, texture, and genesis of Dutch tidal flats and compared them with Upper Devonian deposits in the Belgian Ardennes.

Other invited contributions were delivered by Dr. R. G. S. Hudson, Iraq Petroleum Co., Ltd., on *The Southern Shore of Tethys : an essay in Mesozoic Sedimentation* ; by Dr. E. K. Walton, University of Glasgow, on *Greywacke sedimentation* ; by Dr. W. W. Black, University of Nottingham, on *Carboniferous reef limestones of the North of England* ; and by Dr. G. V. Craig, University of Edinburgh, on *The depositional environment of a Lower Carboniferous marine shale.*

RECENT DEPOSITS AND THE INTERPRETATION OF SEDIMENTARY ROCKS

by

Prof.Dr.Ph. H. Kuenen

IN most branches of geology present-day processes have been investigated to help understand what happened long ago in the earth's crust. ' The present is the key to the past.' In contrast, ancient sediments have been interpreted without serious study of what is going on on the sea floor now. The deep sea, it is true, has received a certain amount of attention. But in spite of the fact that the vast majority of sedimentary rocks were deposited in shallow water, their present-day counterparts now forming around the coasts have been neglected.

For some years the existence of this void has been more generally felt and a growing amount of attention is being focused on the study of recent shallow water sediments.

It is not generally realised that in investigating the floor of shallow seas two quite different aims can be held in view. One can either study the laws of sedimentation, or one can aim at extending the geological map beyond the shore. Different techniques and different specialists are required accordingly.

The laws of sedimentation are the key to understanding the conditions under which ancient deposits were formed. In studying these laws the marine geologist will seek out areas of deposition. The hydrography of the environment, waves, tides, currents, must be studied, together with chemical and biological aspects. In short, a staff of specialists is needed if a complete picture is to be obtained. Samples taken by a simple grab are suitable, because the main concern is with bulk properties of the deposit.

On the other hand the geologist attempting to extend his map beyond the coastline will either prefer areas of non-deposition which will enable him to get at the solid rock, or he will try to probe below the present deposit to reach the covered geological formations. Some kind of coring device or dredge is required to secure samples : examination of the samples can be undertaken as if they were hand specimens or drill cores.

In practice these approaches are not strictly separable but emphasis is generally on one or other of them. In Holland several projects are being carried out in which the study of recent sediments as a key to ancient deposits is the chief aim. Dr. Van Straaten of Groningen University has spent several field seasons on tidal flats and he will himself report on some of the findings.

Pupils of Dr. Doeglas of Wageningen,

especially Mr. Kruit, have worked on the Rhône Delta, mainly on the physiography and shallow borings, but also by sampling on the sea floor to a depth of 100 metres. Some eight different environments have been distinguished, each with a characteristic fauna and grain size composition. Using these results, the borings allow subsurface mapping of former features. Thus the history of the delta since the Ice Age could be roughly reconstructed : the rate of deposition having been about 3 metres per 1,000 years. Interesting phenomena result from flooding during on-shore winds and subsequent drying out of temporary salt lakes. Loss of land is due to lateral transport by waves which erode the shore and the bottom to about 10 metres depth.

Another project, sponsored by the Shell Oil Company of The Hague, was an expedition in 1952 and 1953 to the Orinoco Delta and the area around Trinidad. This region was chosen because it embraces within a limited area not only a large delta partly on an open shelf, partly encroaching on a protected gulf, but also a shelf in the Caribbean and one in the Atlantic.

Among the many important provisional results, mainly the work of Dr. Tj. H. van Andel, are the location of a probable ancient shoreline at about 65 fathoms depth developed during the Ice Age, when sea level stood that much lower. The old course of the Orinoco out to this shore appears to have remained practically uncovered by sediment ever since. All sand carried out to sea by the river is now deposited within a few miles of the coast. Powerful currents sweep the mud into the Gulf of Paria behind Trinidad and a large proportion escapes again on the far side to the Caribbean.

The land surface that formed in this gulf during glacial low levels now lies buried under fine mud and has been identified by echo-sounding. The rate of sedimentation is variable from zero to 1 foot per century, and can be related to currents and wave action. The average rate is equivalent to the formation of 3 inches of shale per century. The large tributary now debouching in the Gulf was established quite recently. At that time the fresh water running out and covering the Gulf during each rainy season suddenly killed off a rich shell fauna on the gulf floor which is now found as decaying shell beds at the surface or just covered by mud. C14 measurements by Prof. de Vries of Groningen showed this catastrophe to have occurred 1,000 years ago. In the sediment the organic matter derived from the Orinoco is found to have a different composition to that provided by planktonic life of the sea, the latter showing a higher percentage of nitrogen.

Sedimentology of the Psammites du Condroz in the Belgian Ardennes [1]

by

Dr. L. M. J. U. van Straaten[2]

The upper part of the Famennian (topmost division of the Belgian Devonian) is developed in the Northern Ardennes as a series of sandstones : the Psammites du Condroz. They have been regarded, since the earliest geological investigations, as typical deposits of neritic or littoral origin. This paper deals with a single group of these strata, the Assise de Montfort, which is easily accessible for detailed studies, owing to the large number of quarries. We may try to define more narrowly the environment in which these sediments were formed. The line of approach employed here is to compare them with a recent littoral formation, for which many data are already available, viz. the Wadden Sea deposits along the south and east coasts of the North Sea. The Wadden Sea is a tidal flat area, the bottom of which is for the most part uncovered at every low tide. As could be expected, no complete correspondence is found (Table I).

Several of the differences mentioned in Table I (Nos. 2, 3, 4, 12) lead to the conclusion that current activity was of comparatively little significance in the Famennian environment. It is not unlikely that the waters were subject to the influence of the tides, but the tidal

[1] A detailed treatment of the same subject has been published in the Dutch geological journal, *Geologie en Mijnbouw*.

[2] The author is indebted to the Netherlands Foundation for Pure Research (Z.W.O.) for defraying the costs of the investigation.

TABLE I

Comparison of Sedimentological Properties

Similarities	*Differences*
(1) General lithology, thickness of beds, mineralogy, grain size distributions.	—
(2) Presence of wash-outs, frequently with bottom deposits of mud pebble conglomerates, shell beds or muddy sediments.	Wash-outs in Wadden Sea more common and of greater dimensions.
(3) Presence of unilateral cross-laminations and of irregular cross-bedding.	Cross-laminations in Wadden deposits more frequent.
(4) Parallel stratification.	Stratification in Wadden deposits usually more irregular.
(5) (Sublenticular) laminations of sand and mud.	—
(6) Ripple bedding.	—
(7) —	Absence (?) of marsh laminations.
(8) —	Abundance of load casts of all sizes in the Psammites du Condroz, as opposed to the rare presence of only the smallest load-cast structures in the Wadden deposits.
(9) Abundance of burrowing structures.	—
(10) Abundance of tracks and trails.	—
(11) Abundance of small-sized mud cracks.	Absence (?) of large-sized mud cracks.
(12) Presence of current ripples, symmetric and asymmetric wave ripples, wave current ripples (?), double wave ripples, interference wave ripple patterns.	Current ripples common and wave ripples rare in Wadden deposits ; abundance of wave ripples and interference patterns in Psammites du Condroz.
(13) Impoverished fauna.	—
(14) Concentration of shells in separate beds.	Absence (?) of shells of Lamellibranchs in living position.
(15) Abundance of drifted plant remains.	

range was probably small. Where tides are weak, along present-day coasts, it is often found, that lagoons are formed instead of true tidal flat areas. As a matter of fact, a lagoon origin seems to agree more closely with the character of the Montfort beds : in these a considerable part of the sediment would seem to have been laid down under a permanent covering of water. Up till now, no clear indication has been found of deposition above the level of the lowest tides. The presence of (small sized) mud cracks cannot be used as an argument for temporary emergence, since cracks of the same type were encountered in channel floor deposits of the Wadden Sea. The abundance of wave ripple marks and the soft state of the mud at the time of deposition (as proven by the load cast structures) can be understood more easily on the lagoon hypothesis. While part of the sediment may have been brought in from the open sea the basin(s) received an important supply of fluvial material from the other side. The beds with unilateral cross

laminations (with laminae inclined predominantly in southerly directions, away from the Old Red continent) can be interpreted without difficulty, as thin sheets of subdelta's, built outward over the lagoon floors.

The assumption that the bulk of the Assise de Montfort was deposited under a permanent cover of water does not imply that tidal flats were absent most of the time. It seems very likely that such areas, and even salt marshes, existed along the borders of the lagoons during the whole period: but the deposits formed at these higher levels were much more exposed to erosive agencies. The majority of them were probably destroyed soon after their formation.

HOW ANIMALS FIND THEIR WAY ABOUT

At the Liverpool Meeting of the Association, Section D (Zoology) devoted the afternoon of Friday, September 4, 1953, to the subject of how animals find their way about. Dr. H. W. Lissmann, of the University of Cambridge, discussed direction finding in fish. He was followed by Dr. D. I. Williamson, of the Marine Biological Station, Port Erin, who delivered a paper on the landward and seaward movements of the sand-hopper, *Talitrus Saltator*. Dr. J. L. Cloudsley-Thompson, of King's College, London, posed the problems of dispersal in some terrestrial Arthropods and the symposium was concluded by Dr. J. D. Carthy, of Queen Mary College, London, with a review of the rôle of vision in route-finding in insects.

Dr. Carthy has edited the papers, which are printed below in the order in which they were delivered.

Direction Finding in Fish

by

Dr. H. W. Lissmann

Research into the migrations of fish which undertake journeys to distant spawning grounds presents problems of great technical difficulty. Even the first stage, the investigation of the routes they take, is often a tedious process. It is not so long ago that the discovery by Johannes Schmidt of the terminus of eel migration in mid-Atlantic was hailed as a major discovery, and it will require further painstaking research before we begin to know the details of movements of fish. It is not surprising, therefore, that work on the mechanism of the navigational abilities of migrating fish is still in its infancy.

Sensory stimuli which may serve as guiding stimuli will probably be found to vary from species to species and every investigation must primarily be based on a satisfactory knowledge of the fish's sensory range. Good progress in the investigation of the sensory physiology of fish has been made in recent years. Laboratory experiments, specially on the minnow, have provided much information about the function of the eye, the ear and the lateral line. There is, however, little to suggest that these organs may be endowed with any exceptional faculties. Examination of the chemical senses and of the temperature sense indicates a great sensitivity compared with our own, e.g. to rose oil the minnow is 250 times as sensitive as man, to cane sugar 512 times, to salt 184 times. It has also been shown that fish can respond to temperature changes of between 0·03° C. to 0·1° C.

Many attempts have been made to deduce from the hydrographical conditions which exist along the routes of migration any gradient of stimulation which may provide the road signs for the fish on their journey. For instance, it is known that not only the European and North American eels, but also species from Australia, New Zealand and Japan breed in warm water of relatively high salinity. From the breeding ground a warm current of water sweeps towards the continents inhabited by the eels. However, when we try to apply to all available data our knowledge of the sensory physiology of fish we come up against some major difficulties. If we accept that an eel may respond to temperature changes of 0·03° C. we do not know accurately how sudden such changes would have to be to be appreciated by the fish. Assuming that for every 100 metres an eel swims it can perceive this temperature difference of 0·03° C., then starting from a temperature of 10° C. it would find itself after 100 kilometres in water of 40° C.—in which it could not live. Even if we grant that eels may be ten times as sensitive as the most sensitive fish we know, most of them would still not get half-way to the breeding ground. Similar calculations can be made for salinity.

Moreover, if both temperature and salinity are considered as guiding factors, all the eels in the Mediterranean would remain permanently trapped. This argument also applies for the gradient of hydrostatic pressure.

There are other physiological enigmas, such as the tagging experiments which have shown that eels travelling from Finland to the south coast of Sweden in 20 to 90 days lose 75 to 150 gr. in body weight. It is clear, therefore, that if the majority of eels continue to travel and lose weight at that rate only a negative quantity would arrive at the breeding ground for, as is well known, the migrating silver eel does not feed and its digestive tract degenerates. This fact, however, may provide a positive clue because it seems to indicate that the remarkable transformation of sense organs which sets in with migration does not serve to secure food but either to find the way or to find a mate. The most striking of these transformations affects the eyes which begin to swell and open up like a bud until, in some specimens which have been caught in the sea, they extend over the greater part of the lateral surface of the head. There are observers who are inclined to ascribe to the eye of the migrating eel a purely negative function because on their seaward journey the eels move only at night, predominantly on moonless nights and on a rising flood. How these two factors affect the fish is not clear, since it is reported that eels kept in tanks in the laboratory are particularly active on the same nights that migration occurs in the field. This observation requires further attention and seems to offer the most hopeful approach for an experimental solution of the problem. It provides a striking parallel to the activity of caged migratory birds which has been shown to contain a marked directional component affected by the position of the sun and the time of day. In the case of birds it has also been concluded that there must be an inherited tendency to migrate in a certain direction, because birds captured while migrating and transported in an easterly or westerly direction tend to maintain their original course when released. No strictly comparable experiments have been performed with eels although Schäffer reports some promising observations which should be repeated and extended. He caught silver eels off the Swedish coast, transported them several kilometres inland and, having released them on damp meadows, noted that each batch of fish behaved consistently by moving off in a definite direction : though not the direction of migration in the sea. When the heads of the eels were set on another course they turned round and obstinately pursued the original direction.

The theory of an ' inherited ' directional tendency cannot be accepted without qualifications in the case of migrating fish. This has emerged from the extensive work which has been carried out on the salmon. In broad outline the life histories of the various species of ' salmon ' are similar in that these fish spawn in rivers, the young migrate to the sea where they live for a variable number of years and return to the rivers for spawning ; whereupon they either die or repeat the process. So far as direction finding is concerned the great interest inherent in this life story appeared when it was claimed that the salmon always returns to spawn in its native stream. Many large-scale tagging experiments have been undertaken and, on balance, it appears that the evidence in favour of the ' parent stream theory ' is fairly good. Although there may be a certain amount of straying, the vast majority of marked salmon make for ' home.' There is clear evidence that salmon undertake journeys through the sea extending over many hundreds of miles. However, it has also been argued that the fish do not usually leave the zone of influence of their river : if they do they are either lost or find their way back by accident. As an acceptable proof of the contrary it was postulated that (i) the young fish should be marked in its native river and liberated ; (ii) that it should be caught and marked again far out at sea ; (iii) that it should be recaptured in its own river on the way to the spawning ground. It is surprising that it has been possible to furnish this difficult proof at all and the few cases proved make the finding of the parent stream by random movements extremely unlikely. Any idea of an ' inherited sense of orientation ' must be dispelled as being equally unlikely because

when eggs from one river were taken to another, where they were hatched and the young fish marked and liberated, they returned after some years to the second stream. This leaves us not only with the problem by what delicate sense a salmon may recognise its own river but also with the assumption that it must remember over a period of several years while it is at sea some impression of that river gained in early youth.

There can be little doubt that salmon are extremely sensitive to minute quantities of chemical substances dissolved in water : this has been strikingly demonstrated by dipping a hand for a short time into the water above a fish ladder over which the fish were running. This not only stopped the upstream migration in a spectacular manner but on occasions even induced the fish to move downstream. Moreover, it is known that some species of fish can distinguish the water of two different rivers by the odorous substances they contain. Such odorous substances can be derived from various sources, including aquatic plants and animals. Indeed, the minnow has been shown to be capable of discriminating by smell not only considerable numbers of fish species and amphibia, but also of distinguishing between two individuals of its own species, even if they are of the same age and sex. Whether this means that each stock of salmon can do likewise, only future work can decide.

Landward and Seaward Movements of the Sand-hopper, *Talitrus saltator*

by

Dr. D. I. Williamson

Talitrus saltator is an amphipod crustacean which, although it is confined to sandy beaches, leads an almost entirely terrestrial life. By day it is normally to be found buried in the sand near high-water mark, but at night it wanders over as much of the beach as is exposed below high-water and some distance above high-water mark as well. Sand-hoppers evidently have some means of finding their way back to the high-water region, and this paper gives some insight into the methods they appear to use.

If, by day, a sand-hopper is taken from high-water mark and released nearer the sea on firm, moist sand it will very soon take up a straight course back to high-water mark, either walking or hopping over the sand(1). Under favourable conditions with no obstructions on the beach and little wind, the track is very straight, not deviating as much as one yard to either side while covering 100 yards of beach.

On reaching high-water mark the hoppers burrow in the softer sand or take cover under wrack. They will take cover before reaching high-water mark if a suitable opportunity presents itself.

When a sand-hopper whose eyes have been painted with a mixture of glue and Indian ink is released on the beach it follows a very convoluted path which shows no directional orientation, but immediately its eyes are cleaned it will make straight for high-water mark. Obviously the reaction is visual.

With only one eye painted the landward reaction is shown as in normal specimens ; the orientation does not involve balance between the two eyes.

Wherever they are collected, specimens released on the same part of the beach follow parallel courses ; the reaction is not homing, and the animals appear to make for some point well above high-water.

The courses followed from certain release points are not quite at right angles to the sea. Specimens may walk away from the sea without jumping or turning. Since the forward position of the eyes must prevent their seeing the sea, and since they are not affected by banks and ridges on the beach which would make the sea invisible, they do not take their direction from the sea.

The hoppers are not affected by overcast conditions ; they do not, therefore, take their direction from the sun.

If transferred to a new beach facing in a different direction the hoppers take up the new landward direction.

A hopper will alter course if a large object (such as a sheet of cardboard) is placed between it and high-water mark, and usually makes for the edge of the obstruction.

From these observations it seems probable that the orientation is to some object well beyond high-water mark, probably to

some feature of the skyline, but this is not always the highest point, and no definite feature has been identified as controlling the migration.

This theory assumes that sand-hoppers have some degree of form vision, and this assumption was substantiated in the laboratory by projecting various shapes painted on lantern slides and recording the tracks of specimens released in front of the screen. They normally made towards a vertical dark-light boundary, or if several such boundaries were presented they would zigzag as they made first for one of them then another. They gave very consistent reactions to slopes when projected as light above and dark below, always making for the foot of the incline, but they showed no directional orientation when the slope was projected as dark over light.

We may summarise thus far by saying that sand-hoppers are capable of some measure of form vision and can probably use this faculty in moving landward over a beach.

Now let us consider sand-hoppers released *above* high-water mark. In my earlier observations I failed to get consistent results on this, but it now appears that these inconsistencies were the result of our inconsistent weather. Professor Pardi and Dr. Papi (2), however, did get consistent results with the same species, working near Pisa on the Italian west coast. They found that hoppers released above high-water mark normally made their way towards the sea, and that this same orientation was shown if the animals were placed in the bowl of a glass bell-jar : they collected on the seaward side, facing seaward and trying to climb the side of the bowl. Further, the hoppers continued to face west in the bowl even when it was carried out over the sea, or taken several miles inland, and even when taken across Italy to the east coast.

They then found that it was not necessary for the bowl to be transparent ; the same results were obtained with a porcelain bowl as long as the sun was allowed to shine on the hoppers. The rôle of the sun was demonstrated by shading the hoppers from its direct light and using a mirror to present a reflected sun to them at different angles. The hoppers took up the same bearing to the reflected sun as they had originally shown to the true sun. They were, in fact, showing a light compass reaction to the sun.

Pardi and Papi also demonstrated that the same orientation was shown if the sun were obscured but a large expanse of blue sky were showing. With the aid of polarising filters they proved that under these conditions sand-hoppers make use of the plane of polarisation of the light from the blue sky, in effect estimating the position of the sun from the polarisation patterns of the sky.

A corollary of this compass reaction is that *Talitrus* must have a time sense, because to get one's bearings from the sun one must know the time of day.

The landward movement described earlier does not require the sun or blue sky to be visible, and the original direction is not maintained if the hoppers are transferred to another beach facing in another direction. The two reactions involve two different forms of navigation : to go landward the sand-hopper seems to rely on coastal features, but to go seaward it navigates by the sun.

During the sunny intervals this summer I have confirmed Pardi and Papi's results, and gone some way to discovering the conditions under which a sand-hopper will show each of the reactions described.

The landward orientation is shown on moist sand on the beach, whether the sun is shining or not. Occasionally specimens released on moist sand show a seaward reaction at first, but these change to the other orientation within two minutes. If the sand is moist above high-water mark (as after rain) hoppers will frequently show a landward reaction here too. The sand in this region, however, is usually much softer than that between tidemarks, and the animals soon burrow.

The seaward orientation is shown when the sun is shining and the hopper is on a dry surface : dry sand, or dry glass, porcelain, etc., in a bowl. On very hot days the surface sand between tidemarks as well as above may become dry, and under these conditions a hopper may show a seaward orientation on any part of the beach, and even walk into the sea. The seaward reaction in the bell-jar is usually destroyed by wetting the sides of the jar or by placing moist sand in it. Only a few specimens,

however, have shown the landward reaction in a bell-jar, and on wetting the sides most specimens either walk round and round the bottom or collect on the side towards the sun, substituting a simple photopositive orientation for the compass reaction.

A sand-hopper does not have a fixed 'seaward' direction which is unalterable for life. Specimens which in a bell-jar in sunlight show an orientation in a fixed compass direction at the time of collection usually lose this ability within two days in the laboratory, and then show an orientation towards the sun whatever its position. Specimens from small bays usually give rather indefinite results in a bell-jar; if a number are collected together perhaps half will show no compass reaction at all, and the others will take up positions facing in several directions on the seaward side of the jar. Experiments on the learning of new seaward directions by sand-hoppers are planned. It seems possible, for instance, that after showing the landward orientation one or more times in sunlight they will then show a seaward reaction in the reciprocal direction, but this remains to be tested.

Returning briefly to the time sense of sand-hoppers, attempts have been made to get more information on this point from their reactions to artificial suns whose positions can be varied at will. One would expect the hoppers to show a different orientation to a fixed artificial sun according to the time of day, so producing a *Talitrus* clock. However, the animals have never been observed to show a compass reaction to any form of artificial light, only a photopositive orientation. A little more success has been achieved using reflections of the true sun, but the weather has severely restricted such observations, and the only conclusion so far is that the elevation of the sun is unimportant.

In conclusion, I should like to stress that there is no suggestion that the two forms of navigation described are the only factors governing the movements and distribution of sand-hoppers. They are merely two mechanisms which tend to return the animals to the high-water region.

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Problems of Dispersal in Some Terrestrial Arthropods

by

Dr. J. L. Cloudsley-Thompson

The terrestrial Arthropoda can be divided ecologically into two main groups. The first of these includes woodlice, centipedes, millipedes and their allies: these lose water rapidly in dry air and consequently are restricted to damp, dark habitats. The second group contains most insects, spiders and other arachnids which are comparatively independent of moist surroundings because their integument possesses an impervious layer of wax that prevents desiccation. It is with the first group that the present communication is concerned.

Investigation of the water relations of the hot-house millipede *Oxidus* (= *Paradesmus*, *Orthomorpha*), indicates that water-loss depends upon temperature and humidity, and is directly related to the saturation deficiency of the atmosphere. There is no evidence of any 'critical' temperature to reveal the presence of an epicuticular wax layer. Comparable results with woodlice and the pill-millipede *Glomeris* show that in general there is a linear relationship between rate of evaporation and saturation deficit. The different isopod species have a considerable range both as regards temperature tolerance and rate of evaporation, and can be arranged in a series from the littoral *Ligia* through *Philoscia*, *Oniscus*, *Porcellio* and *Cylisticus* to the pill-woodlouse *Armadillidium* which has the lowest rate of water-loss per unit area and the highest temperature tolerance.

The absence of an impervious cuticle is probably not a primitive characteristic, but the forms that lack one are so restricted that they cannot be regarded as really successful terrestrial animals. A clue as to the significance of the lack of an epicuticular wax layer is afforded by the fact that in hot weather, woodlice can sometimes be seen moving about in bright sunlight where one would not normally

expect to find them, but where they lose water freely by evaporation, and thus reduce their body temperature below that of their surroundings. Indeed, the absence of a cuticular wax layer combined with the avoidance of dry places probably represents a particular type of specialisation to terrestrial life that may be correlated with low temperature tolerance.

Woodlice aggregate in moist air as a result of the non-directional effects of different humidities upon the rate of their random movements. Such behaviour is called ' kinetic ' and there is no orientation of the axis of the body in relation to the stimulus. The mechanism consists of a decrease in activity and speed coupled with more frequent turnings in moist air. The response is more clearly marked in *Oniscus* than in *Porcellio*, and is least in *Armadillidium*, a sequence no doubt correlated with the differential resistance to desiccation of the three species.

Under normal conditions woodlice avoid light which disturbs them so that they move about and come to rest only in darkness. Indeed, a combination of humidity reactions together with positive tactile and negative light responses serves to retain them in damp, dark places throughout the day. When the air becomes dry, however, the responses of *Oniscus* to light are reversed. Consequently if their day-time hiding places under stones and logs should happen to dry up, the animals do not remain there until they become completely desiccated, but are able to walk in the light until by chance they find some other damp, dark retreat when they again become photo-negative.

The responses of millipedes to environmental stimuli have also been investigated in some detail. With the exception of a directed response, or ' taxis ' away from the light in those forms that possess eyes, and a response to gravity (which, of course, cannot be other than directed since the stimulus is constant) the behaviour reactions of millipedes are entirely non-directional or kinetic. *Oxidus* and the ' spotted snake-millipede ' *Blaniulus* are without eyes but they possess a dermal light sense. When illuminated they crawl around until by chance they find themselves in darkness and again come to rest. Their temperature reactions fall into three categories : there is a general metabolic effect upon the speed of locomotion and duration of the spiral-reflex, and a kinetic ' preference ' for temperatures about 15° C. while sudden drops in temperature engender intense locomotory activity.

Under the influence of drought millipedes move downwards in the soil, but this response disappears in damp surroundings. Airborne odours are apparently not detected, but millipedes are attracted to sugars by means of taste sense organs on their antennae and mouthparts. This point has some economic importance for, under conditions of drought, millipedes may be forced to attack growing crops for the sake of moisture ; and once an attack has been initiated, sugars in the plant sap prevent them from returning to their normal diet of humus and decaying vegetation. It is unlikely that they are the primary cause of damage to crops with tough exteriors such as potatoes and mangolds, for not only are their mouthparts very weak, but they are not attracted to an unbroken surface. Once an entry has been achieved, however, as a result of mechanical injury or via the bite of a wireworm or some other pest, millipedes will eat out the entire centre of a potato and the damage they do is often a prelude to fungal attack.

Reactions to moisture are of greater importance in the lives of these animals (which, with few exceptions, seldom come to rest except upon a damp surface) than are responses to atmospheric humidity gradients. Nevertheless, it has been shown recently that whereas *Oxidus* is very sensitive to differences at the higher end of the humidity range, *Schizophyllum*, which has a much lower rate of water-loss particularly in summer, tends initially to move towards dry air and the reaction is gradually reversed to moist as desiccation proceeds. Again, orientation is entirely kinetic ; speed is greater in dry air than in moist, and in an experimental chamber in which a choice of humidities is provided, both the time spent and the distance covered are greater on the moist side.

Thus although to woodlice and myriapods humidity is the most important factor of the environment, these animals are unable to find their way directly to damp

places. Indeed there is no evidence that they are actually attracted to favourable stimuli at all. They merely avoid places whence they are repelled by unfavourable conditions of drought, light or temperature ; or from which a favourable stimulus (such as the presence of food) recently experienced is now absent. This stereotyped and curiously negative behaviour is, however, surprisingly effective in preventing the creatures from wandering away from their normal habitats. But it raises the problem of how dispersal can take place, and new habitats become colonised.

There are a number of cases on record of millipedes, sometimes accompanied by centipedes and woodlice, migrating in vast armies. Occasionally they have crossed railways and been squashed in such numbers that locomotives have been impeded and sand has had to be strewn on the lines before their driving wheels would grip. At other times cattle have refused to graze on invaded pastures, wells have been filled with drowned corpses and workmen cultivating fields have become nauseated and dizzy from the odour of millipedes crushed by their hoes.

Although such mass migrations (when the normal orienting responses appear to be temporarily in abeyance) might provide an answer to the problem of dispersal, they are of rare occurrence, local in extent and the majority of migrants perish. Consequently the net effect on distribution is probably negligible. We must therefore look for a solution that is more generally applicable and does not involve such abnormalities of behaviour.

An explanation of the problem has been suggested as a result of recent work which has shown that centipedes, millipedes and woodlice are nocturnal in habit and become active during the night when the temperature drops and the humidity of the air rises. At the same time, too, the intensity of their reactions to humidity decreases, at any rate until the animals become somewhat desiccated, and this relaxation of the restraining mechanism enables them to walk in drier places than those in which they pass the day. In addition, enemies are fewer and more easily avoided at night. Thus it is by means of their night-life that these creatures are able to disperse themselves, and overcome the restrictions inherent in the physiology of their integuments.

The Rôle of Vision in Route-finding in Insects

by

Dr. J. D. Carthy

In the main, insects have a well-developed light sense. Since the majority are flying animals, this is not surprising, for this sense is the most efficient for the guidance of animals moving through the air. But comparatively few insects make their way to definite goals, like flowers of particular shapes and colour, and fewer still find these goals by definite routes selected by the individual itself.

The most obvious animal routes are those followed by large numbers of animals at one time and these are usually the migration paths followed in seasonal movements. Butterflies and locusts are outstanding examples among the insects. We know nothing of the way in which butterflies navigate on the routes which have been mapped. But more is known of the movements of locusts. At one time the long distance movements of flying adults appeared to be undertaken in the face of the prevailing winds, the movements of the swarms south-westward through East Africa being contrary to the prevailing south-westerly winds of the season. Closer inspection of wind direction at particular times has shown that the swarms are in fact often travelling with the wind, when they are flying. But they do not fly under all conditions. If the wind is strong and against them the swarms settle. This seems to be brought about by the reversal of the movement of images over the eye as the insects are forced backwards by the head wind. Similarly, drift due to strong winds on other quarters is compensated for by changes in heading resulting from the alteration of the image movement—from front to back in normal forward flight in still conditions, and at some other angle when the insect is drifting. But there remains the problem of choosing the right winds. These may be chosen by the warmth and humidity associated with the north-easterly wind

following the migrants' course. Observations of swarms does not always show a down-wind tendency, except when maps of long-range migrations are made in certain areas. For aero-dynamic reasons a locust takes off upwind. This direction may be maintained for some time by means of the movement of visual images and a light compass orientation to the sun. Thus a single insect or a single swarm will maintain its direction efficiently for a time. Although viewed overall the total movement for the weeks of their migration will be a trend in one direction, each individual section of the path will not necessarily be on the same heading.

The young stages, the hoppers, travel overland by walking interspersed with occasional hops. This movement, too, is orientated mainly with reference to the sun. If the sun is reflected on to marching hoppers from a false direction, the insects change their direction to align themselves according to the artificial new position. But the sight of their companions moving also induces marching.

But this is not true route-finding, for finding implies a selective process which is absent in this form of navigation. The locust apparently does not select conditions but orientates itself automatically to sources of stimuli, a concatenation of which have in the past led to good conditions for reproduction. So this advantageous though—metaphorically—blind behaviour has survival value.

Actual route selection is most highly developed among those insects whose young require provisions, either placed with the egg or supplied progressively as the young develop. Insects showing these forms of parental care occur almost entirely among the Hymenoptera.

Many of the solitary wasps make a nest in which they store food for the larva. The collection of food entails journeying away from and back to the nest, in the search for prey and its transport back. The most detailed investigations have been made on *Philanthus triangulum*, *Ammophila campestris*, and *Bembex rostrata*. Observers had noticed that disturbance of the plants and objects in the vicinity of nests of solitary wasps of all kinds disorientated returning insects. They were also known to have a definite locality sense. It was on these observations that the experiments were based.

The entrance of a nest of *Philanthus triangulum* was surrounded by a ring of conspicuous objects : sixteen pine cones in one series of experiments. When the wasp had left the burrow to seek the bees which it leaves as food for its larvae, the ring of objects was moved sideways and an imitation nest hole made in the centre of it. The wasp's return was observed to see whether it chose to land on the old nest position, or on the new false one. In this way it was shown that objects in the immediate vicinity of the nest were recognised as landmarks. The characteristics of the objects selected as beacons were three-dimensionality and a disrupted outline, though *B. rostrata* is apparently influenced mainly by total surface area. In *Philanthus*, the objects are not perceived as individual pieces of the ring pattern, for a ring of fir cones was attractive while the same cones thrown in a heap were not. The pattern is perceived as a ' Gestalt ' in which properties of the individual objects are subservient to the whole pattern. Further, the whole pattern is perceived in reference to the surroundings, so that moving a more distant landmark may alter the meaning of a pattern. Thus the situation of a flat square piece of wood placed with one point by the nest, with the diagonal continuing over the hole to a distant shrub could be perceived as a whole by the wasp. If the square was turned so that one side now lay next to the nest, the wasp searched both corners on the shrub side. But if the shrub was then moved so that it stood once again on a continuation of one diagonal, only the corner towards the shrub was searched. Clearly the relation of shrub to square was important.

The wasp, *Ammophila campestris*, flies out to the hunting grounds and returns dragging its caterpillar prey, if it is too heavy to carry on the wing. This seemed to present a difficult problem in orientation until it was seen that the hunter flies low at the level of the heads of the heath bushes, and that on its return dragging the prey it frequently climbs to the top of the heath bushes, appears to take a bearing, and then descends once again to travel over the ground for some distance before taking another sight. It is not surprising

that large landmarks are used when some distance from the nest, the size of beacon decreasing as the nest entrance is approached. Gradually a wide knowledge of the hunting area is built up. The allied species, *Ammophila pubescens*, is able to make a detour around a screen placed in its path, returning to the direct track afterwards. But *A. campestris* seems to have a patchy knowledge of its surroundings, for return from one point to the nest may be direct but from only a few feet further away it may return by a wide detour. It would appear that unless the ground has actually been covered, it cannot be counted as known. The wasp has not the power ' to put two and two together.'

The problem arises as to when all this visual knowledge of the surroundings is acquired. Most of these solitary wasps make an orientation flight on leaving the nest entrance of a newly dug hole, or after a spell of bad weather. At this time the immediate vicinity of the nest is related to the more distant surroundings. But the objects immediately around the nest hole are perceived either as the wasp enters the nest or as it leaves it ; for this would explain the preference for raised objects, the unimportance of the size of shadow cast by the objects, and the preference for cubes placed 12 cms. from the nest over similar cubes placed 24 cms. away, a preference which is not apparent if the more distant cubes are doubled in height. *B. rostrata* seems to pay most attention to objects within the sight of the insect while digging. The digging phase is the educative phase in *A. campestris* also.

Very similar orientation flights are known in the honey-bee. On leaving the hive a forager may circle vertically with its head towards the hive. This is apparently the establishment of hive position. The food place characteristics are acquired on the approach to the food, while its position in relation to the surroundings is perceived as the bee performs an orientation flight on leaving.

The designs which a bee can recognise as marking its food place have similar qualities to the objects recognised by the wasps. ' Flicker ' value is most important, that is, a well-disrupted pattern. The flight path may be above definite objects, which when they are moved cause the path to be moved as well. As the hunting wasps have a wide territory learned by flying over it so does the foraging bee.

Less information is available about the ability of ants to use objects to orientate themselves. Certainly white cards moved round the edge of an arena in which an ant is running do not always cause disturbance, except when the area of the card is large. Almost certainly, however, it could be proved that in nature visual landmarks do play a part.

But one method of orientation is not the sole one, for it is clear that the method in use is fitted to the conditions under which it is to be used ; the *Ammophila* running back over the ground, with its prey cannot see the landmarks which it sighted from the tops of a heather clump, so it must be dependent upon kinaesthetic clues to maintain its heading. Similarly if sight of distant landmarks is excluded from the eyes of a foraging worker of *Myrmica rubra*, it can still make its way back to the nest. It is then dependent on the polarised light pattern from the sky. This pattern correlated as it is with the sun's position is now known to be used as the orientation beacon by several ants and the honey-bee, among the insects.

Only in the honey-bee has any detailed study been made of the function of the eye as an analyser of polarised light. For different parts of the sky different patterns will appear in the retinulae cells. Recently, *Drosophila melanogaster* has been shown to align itself along or at right angles to the plane of the polarised light illuminating the fly from vertically above. It has been suggested that this alignment is the fly's known telotactic response to equal strong illumination in both eyes which would arise from a purely physical consideration of the amount of light refracted into the ommatidium when polarised light strikes the lens at different angles. From such a calculation it can be shown that the two positions mentioned will give the effect of equal strong illumination in the frontal ommatidia of the two eyes. Thus no analysing mechanism has to be postulated. There are no signs that the fly uses polarised light for orientation under any conditions but these special ones.

This raises the problem of the use of the

polarised light pattern by ants and bees. There is no evidence of any means of communication of routes by ants, and in this they differ from the honey-bee in which a whole complex of communicating dances has been demonstrated. There seems no reason therefore why the pattern used by the ant should not be memorised, or 'engrammed' on the outward journey, and reversed in retinal position for the return. There is no necessity to suppose that the forager determines the position of the sun from the polarised light pattern—as indeed theoretically it could. But the honey-bee indicates the position of the food with respect to the sun in its dance and therefore must use the pattern to derive the position of the sun. Similarly, the forager following the dance must, if the sun is hidden, deduce from the information given in the dance the particular pattern which it must maintain in a particular part of its eye to make good that bearing. This implies a more complex central mechanism than in the ant. For the ant the polarised light pattern is probably only one of the methods of maintaining a direction for short distances, reinforcing information from olfactory, and perhaps kinaesthetic sources.

We have so far considered the form of the information, but little attention has been given to the way in which it is acquired. Whatever fixed points are utilised, sky light-pattern or objects on the ground, the storing of these in the memory is the result of a learning process which occurs quickly. The information learned on an orientation flight is complex and yet it is completed in times from 3 to 100 secs. in *Philanthus triangulum*. The knowledge is retained for considerable periods : in bees for as long as seven days. As the hunting area is increased so more and more land becomes known, and the problem of selecting from the memory traces in order to orientate becomes more and more difficult. It must be remembered that memory traces from the other senses are also stored. It is this sort of central nervous process which has so much interest attached to it, for it is plain that the visual world of the route-following insects is highly complex.

THE HEAVY CHEMICAL INDUSTRIES OF MERSEYSIDE

At the Liverpool Meeting of the Association, Section B (Chemistry) devoted the morning of Friday, September 4, to a series of papers on the heavy chemical industry of Merseyside. Dr. J. Ferguson, Imperial Chemical Industries, Ltd., presented a general and historical survey which is printed in full below. There were three other invited contributions to this session. Mr. J. H. Harwood, Messrs. Peter Spence and Sons, Ltd., reviewed the development from ancient times of the alum and aluminium sulphate industries and described modern plant processes. Dr. T. Kennedy, Joseph Crosfield and Sons, Ltd., discussed the growth of the soap and allied industries with special reference to silicates, edible oils, and synthetic detergents. Finally, Mr. A. J. Carter, Shell Refining & Marketing Co., Ltd., gave an account of recent developments in the petroleum industry which emphasised Merseyside's contribution to Britain's post-war increase in oil refining capacity.

The Heavy Chemical Industries of Merseyside

by

Dr. J. Ferguson

Merseyside, together with the neighbouring Cheshire saltfield, is the most important area in Great Britain for the manufacture of heavy chemicals, and one of the most important in the world. This has been so for well over a century. Indeed, at one time during the Victorian period, before the Industrial Chemical Revolution had advanced far in other countries, Merseyside was the world centre of gravity of heavy chemical manufacture.

By heavy chemicals we mean substances of simple chemical structure, manufactured in large tonnage from relatively simple raw materials and usually shipped in bulk to industrial or agricultural consumers. Examples of such chemicals are sulphuric acid, alkalis, bleaching powder, chlorine, superphosphate, and a number of inorganic salts such as alum and copper sulphate. Nowadays we might add to this list certain organic substances of simple structure, also made in large quantity.

All the chemicals in the above list and many others have been, and are to-day, made in the chemical towns of the Mersey area. It is of interest to trace the history of this industry, to discover how it began, why it developed successfully, to portray the changes in its pattern with the passage of time, and to survey its present structure.

Merseyside was not the birthplace of heavy chemical manufacture in Britain. In the sixteenth century, long before chemical manufacture settled in the Mersey area, the heavy chemical, alum, was made in Yorkshire by an extractive process from shales. The manufacture of sulphuric acid by the lead chamber method began in Birmingham in 1746, when Roebuck erected there his 6-foot cube chambers. During the latter half of the eighteenth century other small sulphuric acid works arose in various parts of the country. The large scale manufacture of bleaching powder, which subsequently became of great importance on Merseyside, originated in the famous works of Charles Tennant, erected in 1798 at St. Rollox, Glasgow. Even Leblanc soda, a manufacture with which the name of Merseyside will always be associated, seems to have been first made in Britain at Tennant's works in Glasgow in 1818, and in the same year on the north-east coast by William Losh and Lord Dundonald. These particular enterprises, however, operated on a small scale.

The Merseyside heavy chemical developments which ultimately became so important, originated in 1822–23. In 1822, James Muspratt, the effective founder of the British alkali industry, erected sulphuric acid chambers of hitherto unprecedented size (112 feet × 24 feet) at 289, Vauxhall Road, Liverpool, and in the following year, 1823, began in those works to make alkali by the Leblanc process. In so doing he initiated the main stream of development of British heavy chemical industry, a stream which, much transmuted, continues to-day to expand and diversify. If Merseyside cannot claim to be the birthplace of heavy chemical *manufacture*, it may claim to be the birthplace of heavy chemical *industry*, for it was on Merseyside that the Leblanc complex of manufactures first became well established and attained its greatest expansion, and it is surely right to regard the Leblanc period as the first great era in the evolution of modern heavy chemical industry.

It will be recalled that this famous process was invented by the Frenchman Nicholas Leblanc, during the French revolutionary period, to supply a method of making alkali from common salt which would be capable of meeting the increasing demands for alkali in the manufacture of soap and glass. Until the Leblanc process became established, makers of these commodities had to rely on natural mineral soda or on the calcined ashes of certain plants, such as kelp and the sea-wort.

In outline the Leblanc process consisted of two main steps. First, salt was treated with sulphuric acid to yield anhydrous sodium sulphate or saltcake, with simultaneous evolution of hydrochloric acid. In the second step, the saltcake, mixed with limestone and coal, was heated to yield sodium carbonate and calcium sulphide.

$$2NaCl + H_2SO_4 = Na_2SO_4 + 2HCl$$
$$Na_2SO_4 + 2C + CaCO_3 = Na_2CO_3 + CaS + 2CO_2$$

The solid product of the second step, known as black ash, was at first used directly, but later was extracted with water to obtain sodium carbonate.

The Leblanc process did not in France fulfil the promise of an early rapid development by the soap boilers of the Marseilles area. In Germany it developed late, beginning about 1840, and grew slowly. It was never worked in America. On Merseyside, after 1823, in the hands of such men as Muspratt, Gamble, Kurtz, Gaskell, Deacon and Gossage it grew rapidly. Works were erected at St. Helens, Newton-le-Willows, Widnes and Runcorn, and by the fifties of last century the area in and around Widnes became the chief world producer of Leblanc ash and the associated products of the industry. There were similar but less extensive developments in the Midlands, on the Tyne, and at Glasgow.

In 1840, soda ash production in Great Britain amounted to about 52,000 tons. By 1852 the output capacity of British plants reached 100,000 tons a year of alkali equivalent to sodium carbonate, over 40 per cent. being exported. Ten years later, in 1862, capacity had doubled again, to a figure of 200,000 tons. The peak for Leblanc soda only was reached in the eighties, at a figure of over 400,000 tons a year carbonate equivalent. At that date almost 70 per cent. of the world output of Leblanc alkali was made in Britain.

The great development of the Leblanc system of processes on Merseyside was a natural one. Firstly, there was a large demand for alkali in the area, for it had long been a centre for the manufacture of hard soap. (Until Muspratt began operations Mersey soap-boilers had had to rely on calcined kelp from Scotland or Ireland as their source of alkali.) There were the neighbouring textile industries of Lancashire and Yorkshire needing soap and alkali for cleansing and scouring, as well as other products of the Leblanc system, such as acid and bleach. There were the glass factories of St. Helens with their demand for sodium carbonate and saltcake.

Not only was there an immediate market for the products of the industry but the region was also well situated as regards ready availability of the raw materials for the Leblanc process, salt, limestone, coal and sulphur or sulphur compounds. Close at hand lie the famed salt deposits of Cheshire, worked before Roman times. Limestone is available in the Derbyshire hills and in North Wales, and coal in Lancashire. Sulphur or pyrites is easily

imported through the great port of Liverpool.

Finally, through this same port, the products of the Leblanc industry could be exported to the ends of the earth. There were many products besides the main one, sodium carbonate, and the caustic soda derived from it by boiling with lime. The Leblanc system of manufacture touched a wide area of classical inorganic chemistry. Though an alkali industry, it required the manufacture of a corresponding tonnage of sulphuric acid, with all that that involved. The pyrites supplying the sulphur for this contained copper, zinc, silver, gold, arsenic, and during the heyday of the system all these were extracted commercially on Merseyside from burnt pyrites.

The first stage of the Leblanc process produced saltcake, important for glass making, while the hydrochloric acid evolved in this same stage, at first a nuisance, gradually became an article of commerce and later a source of chlorine to make bleaching powder which became an important export product. The chlorine required for bleaching powder was obtained at first by the familiar laboratory reaction between manganese dioxide and hydrochloric acid. In 1868 Henry Deacon of Widnes was granted a patent for oxidising hydrochloric acid gas with air over a copper catalyst—a classical example of chemical equilibrium studied by all students of chemistry. In course of time this route, as the route to chlorine, rivalled the manganese dioxide route and the Weldon improvement of it. Both processes ceased to be employed when electrolytic methods came into use.

One great objection to the original Leblanc process, the complete wastage of the sulphur entering the process in the sulphuric acid used, was overcome by the Chance recovery process. This involved treatment of the evil-smelling alkali waste (calcium sulphide) with carbon dioxide to yield H_2S, which was then decomposed in a Klaus kiln to produce sulphur.

The once great and flourishing Leblanc alkali industry, whose smoke in the past darkened the skies of Widnes and Runcorn, has now disappeared. It was killed by two things, first the rise of the ammonia soda process and secondly the development of electrolytic chlorine and caustic soda manufacture. The ammonia soda process, though it employs ammonia in the operating cycle, may be summed up simply by the equation :

$$2NaCl + CaCO_3 = Na_2CO_3 + CaCl_2$$

Gone are all the chemical complications of the Leblanc process. The ammonia soda process needs no sulphuric acid, no sulphur recovery. The chlorine of the entering salt is not recoverable, so gone is the complication of the Deacon process. But gone also are all the Leblanc by-products : saltcake, sulphide, hydrochloric acid, bleach, chlorine. Despite the lack of profitable by-products the ammonia soda route produced a much cheaper and also a purer soda.

An ammonia soda process was patented in England in 1838 by Dyar and Hemming. Unsuccessful attempts to operate it were made by Muspratt at Newton-le-Willows in 1838–40 and by Deacon at Widnes in 1852–54, but its very considerable technical difficulties associated with the recovery of the circulating ammonia, were first surmounted in the sixties by the Belgian gas-works engineer, Ernest Solvay. As is well known, Ludwig Mond, who had studied in the world-famed chemical laboratory at Heidelberg and later practised industrial chemistry in Widnes, introduced the Solvay ammonia soda techniques into England. With his partner, John Brunner, he erected works at Winnington, near Northwich, in the year 1872. A huge soda and lime-caustic factory grew gradually at Northwich and by 1892 over 300,000 tons of ammonia soda were being made in England—more than the simultaneous Leblanc output.

Strong competition ensued between the two processes. Leblanc manufacturers, to meet the threat of the new process, and also to combat growing foreign competition in heavy chemicals, joined forces in 1890 to form the United Alkali Company, with headquarters in the Mersey area. This was the first great merger in the history of the world's chemical industry. Though the United Alkali Company also erected a large ammonia soda works at Fleetwood, they continued to operate the Leblanc system, and the many associated

products enabled the older process to hold its head above water for some time, although by 1914 it was clearly dying in this country. The preoccupations of war allowed it to continue for a brief period, and it finally expired in Great Britain in 1920.

The Leblanc system has, of course, left behind it on Merseyside an important chamber sulphuric acid industry. Apart from this there still remain vestiges—other intermediates of the Leblanc process now made and sold in their own right. Saltcake and saltcake-hydrochloric acid continue to be made, and a typically heavy chemical process, the manufacture of sodium sulphide by reduction of saltcake with coal slack, continues in Widnes. This last is essentially the second stage of the Leblanc process, with the limestone omitted.

What has taken the place on Merseyside of the once great Leblanc complex of processes? There are many diverse heavy chemicals manufactured there but pride of place is taken by two: sulphuric acid and electrolytic chlorine and caustic soda manufacture.

The manufacture of sulphuric acid is widely spread in Great Britain but the main concentration of its manufacture is in North-West England and particularly Merseyside. The acid is made both as an article of commerce and for immediate use on the site of manufacture. Both chamber and contact acid are made, though the latter type of manufacture is nowadays predominant. It is interesting to note that the first contact plant in England, leaving aside a small plant in Scotland, was erected in Widnes in 1915 in the Mathieson Works of the United Alkali Company.

Among the immediate uses of sulphuric acid on Merseyside for heavy chemical manufacture, we may note the manufacture of superphosphate carried on by Messrs. Fisons in their extensive works at Widnes. This business, originally Thomas Vickers & Sons, came to Widnes in 1873. A superphosphate plant, originally built by the United Alkali Company, operated in Runcorn until 1937. Aluminium sulphate manufacture by the action of sulphuric acid on bauxite and clays began in Widnes in 1908. There are now three firms in Widnes engaged in its production—the Alumina Company, the Viaduct Alum Company, and Peter Spence & Sons Ltd. The last-named firm was founded by the famous Peter Spence who established works in Pendleton in 1846 to work his new and revolutionary alum process. The Spence works in Widnes commenced operations in 1920. In addition to aluminium sulphate, alum and sulphuric acid, Messrs. Spence manufacture various titanium salts, activated aluminas, and finely-divided silica. Another manufacture employing sulphuric acid is the production of the white pigment, lithopone. Zinc sulphate solution, made by dissolving zinc residues in sulphuric acid, is mixed with an aqueous solution of barium sulphide. The resulting precipitate of zinc sulphide and barium sulphate is calcined and ground. The process was invented by J. B. Orr, the father of the lithopone industry, in 1874. Orr's Zinc White Company, the oldest and one of the largest firms of lithopone manufacturers in the world, began operations in Widnes in 1898. Lithopone is manufactured also by the Widnes firm of McKechnie Brothers. The manufacture of copper sulphate for use in viticulture is an important sulphuric acid consumer. This product is a by-product of an important copper refining industry in Widnes and is made by Thomas Bolton & Sons, James H. Dennis & Company, and McKechnie Brothers. Finally, there must not be forgotten the manufacture of saltcake and hydrochloric acid by Imperial Chemical Industries in Runcorn.

The raw materials for sulphuric acid manufacture in the Mersey area are at present brimstone, pyrites and gas-works spent oxide. The increasing world demand for sulphur and the foreseeable exhaustion of the Texas fields have resulted in an increasing use of pyrites and of the indigenous material, spent oxide. A new development in the use of indigenous sulphur material on Merseyside is about to take shape in Widnes. The United Sulphuric Acid Corporation, an association of sulphuric acid users, is at present erecting there a large plant to make sulphuric acid from calcium sulphate which is obtained as anhydrite from Cumberland. The anhydrite, ground

and mixed with coal and sand, is heated in large rotating kilns, where the calcium sulphate decomposes, yielding sulphur dioxide, while its lime content reacts with the sand to form cement clinker. The gases from the kiln pass to a contact plant for conversion to sulphur trioxide. At present there is one anhydrite cement-sulphuric acid plant in the country : the plant at Billingham erected in 1928.

Before leaving the subject of manufacture of sulphur compounds it should be mentioned that, associated with the Merseyside sulphuric acid plants, there is also production of such salts as sodium sulphite, bisulphite, thiosulphate, etc. Carbon disulphide has been made in the district for fifty years, and there is a very important hydrosulphite production at Messrs. Brotherton's works near Bromborough, Cheshire. Sulphur dioxide, for sale in liquefied form, is among the many other products made by this firm.

Turning now to the second great chemical industry on Merseyside—electrolytic caustic and chlorine—we must realise that this began essentially as an alkali producer. The evolution of reliable dynamo-electric machines in the 1870's, capable of supplying heavy direct current, turned the thoughts of inventors in the later years of the nineteenth century to electrolytic methods of alkali manufacture.

In passing, we may note the experiments of Hargreaves and Bird in Widnes in 1890–95, which resulted in their well-known vertical drained cathode diaphragm cell, at one time much used. But the main developments in the Merseyside area arose from the work of Hamilton Young Castner, on the mercury cell process. In 1897, supported by the great technical experience and the financial backing of the Manchester engineering firm of Mather & Platt, the Castner-Kellner Works were erected near Runcorn, to exploit the mercury cell process of Castner and his associate, Kellner. In these works Cheshire brine is electrolysed to yield chlorine, aqueous caustic soda, and hydrogen, in a factory which has grown considerably in size since its inception. The first mercury cells to be installed there were the famous ' rocking cells '—often pictured in chemical text-books. Though this type lingers on in some other parts of the world, it became effectively obsolete about fifty years ago. The cells installed in 1902 in the Castner-Kellner Works, and all others since, are stationary cells and consist of very long, slightly inclined troughs, through which mercury is circulated by means of a pump.

Other electrolytic chlorine works, all operated by Imperial Chemical Industries, exist in the Widnes-Runcorn area, one of which is a diaphragm cell installation erected in 1915 by the United Alkali Company. The electrical energy required for these works is generated in two coal-burning power stations the combined capacity of which considerably exceeds 100,000 kW. Not all the power available, however, is used in electrolysis.

As mentioned above, the electrolytic production of chlorine is a triple-product process. The caustic liquor produced is either sold as such or is concentrated in cast iron pots to molten anhydrous caustic soda, which is cast in drums for transport. At one works some of the hydrogen evolved is used to make synthetic ammonia. A much larger proportion is combined with chlorine, in hydrochloric acid burners, the exit gases being absorbed in water to yield the common commercial grades of hydrochloric acid, containing 28 per cent. or 36 per cent. of HCl.

This manufacture of synthetic hydrochloric acid consumes only a relatively small proportion of the chlorine produced. A large quantity of chlorine is liquefied and transported in this form for sale in 14-ton capacity rail tanks or in road tanks. Liquid chlorine was first made industrially in this country at Runcorn in 1908. Some chlorine is converted on site into sodium hypochlorite solution, and some into bleaching powder. The latter substance is not now manufactured in chambers, with their attendant unpleasant heavy manual labour, but in long rotating tubes. The manufacture of bleaching powder, however, once a mainstay of the Leblanc process, and an important export from Merseyside, is to-day conducted on a much reduced scale. Modern bleaching is largely effected by means of calcium hypochlorite solution, made on site from liquid chlorine and lime slurry or with purchased sodium hypochlorite solution.

The greater proportion of chlorine made is used for the manufacture of organic chemicals, either on site or after transport as liquid chlorine to other factories. Among those made on Merseyside may be mentioned trichloroethylene, used as a degreasing solvent in the engineering trades and to a lesser extent for dry-cleaning and in the extraction of edible oils. Tetrachloroethane, made by the union of acetylene and chlorine, is the starting point in the manufacture of trichloroethylene and of other chlorohydrocarbon solvents. The processes have operated since 1909. Other important chlorine derivatives made in the area are benzene hexachloride, the well-known insecticide, 'Methoxone' a hormone weedkiller, vinyl chloride, monochlorobenzene, methylene chloride, chloroform, and carbon tetrachloride. The last named is manufactured by Messrs. Albright & Wilson, by the interaction of chlorine and carbon disulphide.

In the nineties, when the electrolytic alkali industry began, caustic soda was the product sought. Chlorine was a useful by-product but subject to over-supply, a fact which limited the expansion of the electrolytic alkali industry. The picture has now changed completely. Over the years, the demand for chlorine grew with increasing momentum, until after World War II it began to soar. This great expansion has been due to the increasing use of chlorine in the manufacture of organic chemicals, which completely overshadows its consumption in bleaching and oxidising processes. The effect of the greatly increasing output of chlorine, with simultaneous production of caustic soda, will obviously have marked repercussions on the other route to caustic soda—the causticisation of sodium carbonate with lime.

Electrolytic caustic and chlorine from brine are not the only electrochemical manufactures on Merseyside. Metallic sodium is produced by the electrolysis of molten sodium chloride in one works. A second installation on another site will shortly come into operation. The sodium is used in the manufacture of sodium peroxide and sodium perborate, and in the production of lead tetraethyl—the anti-knock.

A possible future electrolytic development on Merseyside is the manufacture of fluorine. Industrial uses of elementary fluorine, however, have not so far developed greatly and fluorine chemistry in the area is at the moment most strongly represented by the production of fluorine-containing refrigerants such as dichlorodifluoromethane and similar compounds made by the interaction of hydrofluoric acid and chlorinated hydrocarbons.

Electrothermal chemical operations are carried on by Imperial Chemical Industries and by Messrs. Albright & Wilson. Since 1940 the former firm has operated a calcium carbide plant in the Runcorn district, the product being used to generate acetylene for the manufacture of chlorinated solvents and vinyl chloride.

The electrothermal manufacture of elemental phosphorus has been conducted in Widnes since 1933 by Messrs. Albright & Wilson who, in subsequent operations, convert the element into phosphoric acid and thence into various calcium and sodium phosphates. The latter find important uses in water treatment and the modern detergent industry.

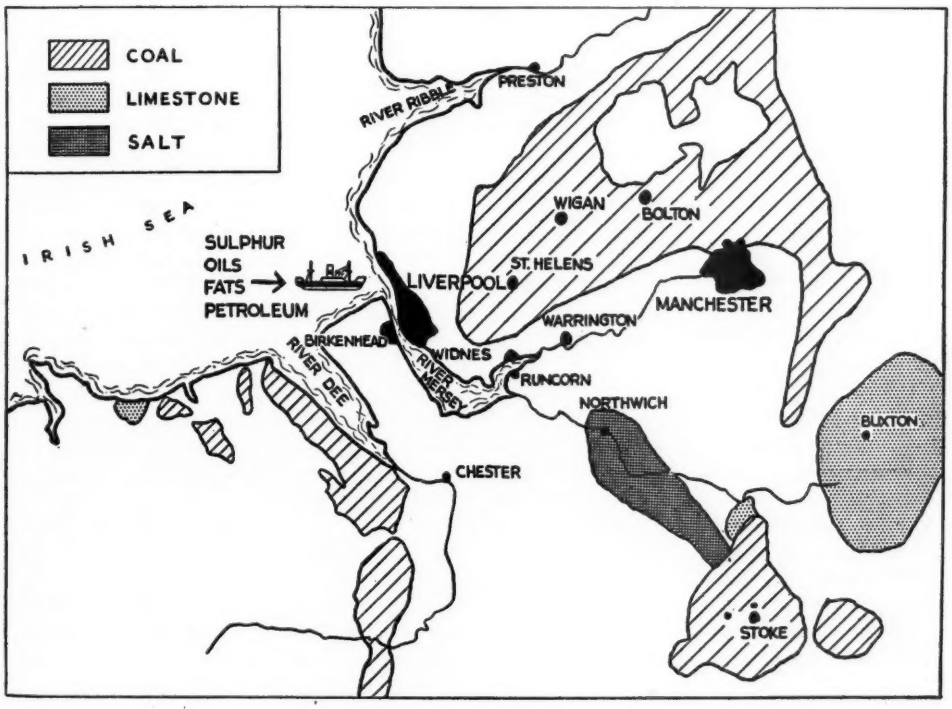
In the enumeration of Merseyside heavy chemical industries we should not forget the production of salt—a physical rather than a chemical process. While this manufacture is, of course, widespread in the area around Northwich, an important vacuum-evaporation plant exists in Runcorn, operating on Cheshire brine.

Finally, reference must be made to the manufacture of a really heavy chemical: white lead. Practically the entire British production of white lead, amounting to about 16,000 tons per annum, is concentrated on Merseyside, in the works of Associated Lead Manufacturers in Bootle and of the Mersey White Lead Company at Warrington. The former works are the largest in Europe and the second largest in the world. Both firms use the same modern mechanised and highly efficient process—the 'Librex' process, developed originally in the twenties in Runcorn by the Runcorn White Lead Company. By this process, metallic lead is transformed to white lead in a reaction time measured in minutes, compared with the many hours required by the old stack process.

This brief review has dealt mainly with the classical heavy chemicals made on Merseyside. As long as the neighbouring salt, coal and limestone deposits exist, this kind of chemical industry will find a natural home on Merseyside. The recent important developments in oil-refining and petro-chemicals on the south bank of the river have brought a new type of chemical industry to the area. This also may be called a heavy chemical industry and we may confidently expect its continued expansion, side-by-side with its elder brother.

In conclusion, it is proper to record something of the interest shown by the pioneers of the industry in the British Association. When the Association, then only in the seventh year of its existence, met in Liverpool in 1837, its sessions were attended by two of the most important pioneers of the British heavy chemical industry : James Muspratt and Charles Tennant. The latter came down by steamboat from Glasgow for the occasion and stayed with his friend Muspratt. Muspratt and Tennant were not the only chemical industrialists who attended that British Association meeting of long ago : William Losh's brother James, Andrew Kurtz, Josias Gamble, and Tennant's son John all attended at the Mechanics' Institute (now Liverpool Institution) and heard the discussions in the Chemical Section in which Faraday, Graham and Liebig took part. It is significant that the founders of the chemical industry were actively interested in the advancement of science. Their activities in the large scale transformation of matter were conducted in the light of a very small capital of scientific facts established mostly during the eighteenth century, but, small though this scientific capital was, it was essential. The heavy chemical industry of Merseyside could not have arisen before the birth of chemical science.

The highly evolved pure science of today has a much more immediate impact on industrial operations. To assimilate and apply its discoveries and developments, large applied research laboratories have become indispensable adjuncts of modern industry. Many such have arisen on Merseyside. Indeed, the first industrial chemical laboratory in this country,



specifically organised for research, was, it would seem, that erected in Widnes in 1891 by the United Alkali Company and known as the Central Laboratory.

The work of these laboratories is, of course, firmly rooted in pure science and their existence is additional confirmation of the fact that nowhere may the dependence of industry on the progress of pure scientific research and discovery be more convincingly witnessed than on the shores of our great river, where chemistry played its first major part in the industrial revolution.

Acknowledgments

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THE NATURAL HISTORY OF COMMUNICABLE DISEASE

At the British Association's meeting in Liverpool, Section I (Physiology) devoted the morning of Wednesday, September 9, to a discussion of the natural history of communicable disease. Prof. B. G. Maegraith opened the symposium with a paper on the research approach to diseases in hot climates. Prof. R. M. Gordon and Dr. R. B. Griffiths then presented a brief account of the factors which influence the transmission of filariasis to man and his domestic stock. Dr. A. R. D. Adams reviewed the factors which may be responsible for variations in the clinical effects of parasitisation of man by *Entamoeba histolytica*. Dr. W. H. Horner Andrews then dealt with the physiological approach to research projects in malaria with special reference to lesions in the liver, and he was followed by Prof. T. H. Davey, who discussed disease as a factor limiting the growth of population in underdeveloped areas and the need for integrating medical advances with progress in other fields. The symposium concluded with a joint contribution by Prof. A. W. Downie, Dr. K. R. Dumbell and Dr. K. McCarthy concerning observations on the infectivity of smallpox.

All the contributors to the symposium are members of the staff of the University of Liverpool. The papers have been edited by Dr. Dorothy H. Strangeways, The Queen's University, Belfast.

The Research Approach to Diseases in Hot Climates

by

Prof. B. G. Maegraith

The natural history of a communicable disease covers the study of the infective agent, the methods of spread, the effects of the agent on the individual host and the collective effect on the community of hosts. Some of these points will be covered in this symposium.

The study of the infective agent may be carried out *in vitro* under artificial conditions or *in vivo* in a host animal. A knowledge of the agent's reaction to the artificial culture may be of importance in such things as the diagnosis of a communicable disease—for example, smallpox—but it often helps little so far as knowledge of the effect of the agent on either its vector or ultimate host is concerned.

The natural history of a disease thus includes the study of both the action of the parasite on the host and of the host on the parasite. So far as ultimate pathological patterns are concerned, the former has been studied fairly closely, although the more fundamental study of the local and general pathological processes involved in the production of injury to the host has until recently been largely neglected. Some of the work going on in the Liverpool School of Tropical Medicine in this respect, especially in regard to liver damage, will be described during this symposium.

The reaction of the host on the parasite has also been neglected and deserves a great deal more attention than it has so far received. It is becoming clear that the balance host-parasite-host is such that in many ways better results may follow the study of an infective agent in its natural host than the study of an agent artificially supported in an unnatural host.

The study of the infective agent itself has so far been mainly an artificial one. It is commonly difficult to observe the agent in its natural host, so that most observations have had to be made *in vitro* sometimes with organisms artificially separated from their natural environment. New methods are needed in which the organism can be studied *in situ* in its proper host.

The importance of a communicable disease to a community is clearly brought out in the study of virus infections and will be emphasised in regard to smallpox. The ultimate object of the study of communicable disease is presumably its eradication. The removal of the effects of widespread disease affecting a community raises well-known demographic points, and will be discussed later by Professor Davey.

In the pathogenesis of malaria the overall problem is to determine how the invasion of limited numbers of red blood cells can lead to the profound and varied changes occurring in the host during the disease.

We have spent some years studying this problem and have learned amongst other things that many of the disturbances initiated in the host by the parasitic invasion are derived from processes which are not at all specific to malaria but are common to many other pathological states. Dr. Andrews will describe later our views on the production of liver lesions in malaria. The precise part played by the parasite in producing malaria is not yet fully understood. It is generally assumed (though not yet proven) that the asexual parasites developing within the host red blood cells are largely responsible for the condition of the host, and that the tissue forms (arising from mosquito-transmitted parasites) are not. It is from this premise that our work in Liverpool has been developed.

Experience of malaria has shown that local accumulation of parasites is by no means an essential factor in the establishment of local lesions. Moreover, even relatively small numbers of parasites in the blood—for instance, in falciparum malaria in man—may be associated with profound general effects such as vascular failure. It would seem therefore that the parasites must in some way be able to exert remote effects on the host. The simplest explanation would be the existence of some active diffusable chemical agent evoked directly or indirectly by the parasitic invasion. So far no such soluble agent has been demonstrated in any form of malaria.

I think this is because the search has gone on in the wrong place. It is most improbable that the parasite produces some elaborate specific substance which has not yet been demonstrated ; it is much more likely that it exerts its effects through the medium of one or more relatively simple and non-specific physiologically active substances possibly of the type recently isolated in acute inflammatory conditions. The discovery of the means by which the parasite exerts its effects would greatly add to our knowledge of malaria. We have to confess ignorance of the manner in which the parasite initiates and maintains the developing disease. We are equally ignorant of the more direct relations between parasite and host and host and parasite. We have to realise that progress in this field cannot be appreciable until we discover how to differentiate between the metabolism of the parasite and that of the red cell containing it. Some method is needed for studying the food requirements and so on of the parasite as it develops undisturbed in its natural environment within the host red cell in the intact animal. Our recent demonstration in Liverpool of the suppression of malaria parasites by placing their natural hosts on an exclusive milk diet may be important in this respect, since the growth of the parasite can be studied in this way by ringing the changes on the diet of the host without removing the parasite from its environment.

The comparisons of parasite and host metabolism made possible by our present imperfect techniques have so far revealed little evidence of any appreciable direct competition between the two and have indicated that the metabolic pathways in both are broadly the same. Clinical and chemotherapeutic studies, however, indicate that there are fundamental differences. We need new techniques to find them. Perhaps the suppression of malaria by milk may be important here.

In trying to demonstrate to you very simply some of the obvious problems of the host-parasite relationship in malaria, I am afraid I have probably done little more than emphasise our amazing ignorance of the biological background of the disease. I am sorry about this, but after all, ignorance isn't such a bad thing if we recognise it and use it as a stimulus for finding things out.

The Reactions of the Vertebrate and of the Invertebrate Host to Certain Species of Filarioid Nematodes and the Possible Effects of such Reactions on the Spread of Human Filariasis

by

Prof. R. M. Gordon and Mr. R. B. Griffiths

The term filariasis means a state of parasitism caused by helminths occurring in the superfamily Filarioidea.

To the medical practitioner the term filariasis implies infection of the human host by one or more of half-a-dozen species of helminths whose distribution, for all practical purposes, is confined to the tropics, and he divides, more or less sharply, these few species into two groups, one of which he calls pathogenic and the other non-pathogenic. To the veterinary practitioner, the term filariasis has a similar but wider meaning, for he includes in it infection of many widely different types of domestic animals by numerous species of filarioid nematodes whose distribution is not confined to the tropics. Indeed, the occurrence of filariasis in domestic stock is by no means uncommon in Britain; thus Le Roux (1950b) found that a high proportion of horses in the London area were infected with the filarial worm *Onchocerca cervicalis*, which is transmitted by the common biting midge *Culicoides nubeculosus*; while the work of Steward (1935) and Le Roux (1950a) has shown that cattle in Britain are often infected with *O. gutturosa*, which is transmitted to them by the ' black fly ' *Simulium ornatum*.

It remains for the zoologist to take a broader and, it would seem to us, the more reasonable and instructive view of filariasis. To the zoologist, filariasis represents a form of parasitism which is of particular interest since it is of world-wide distribution and since the adult forms occur not only in man and domestic animals, but also in a wide variety of wild animals, birds and reptiles. From these various hosts the zoologist has collected adult worms representing more than 100 distinct species; unquestionably, however, the number of species is greatly in excess of this figure.

The reactions exhibited by the vertebrate host to invasion by widely differing parasites are clearly associated with the life-cycle of the parasites concerned and will differ with different species, but the following observations appear to be of general application to most parasites and most hosts.

In the case of parasitic unicellular organisms the number of protozoa or bacteria introduced is comparatively unimportant since they multiply within the body of the host. Thus the few plague bacilli, trypanosomes, or malaria parasites introduced by, respectively, the flea, the tsetse fly and the mosquito, although at first innocuous, may destroy their host when their reproduction has resulted in millions of parasites.

In the case of the metazoan parasites, however, with a few exceptions, no such multiplication occurs, so that a person exposed to a single bite from an arthropod infected with a helminth parasite cannot at any time harbour more adult worms than the number of larvae introduced at the time of the bite. It is true that these, and other species of helminths, may, when adult, give rise to numerous first stage larvae, but these have no power of further development or multiplication within the vertebrate host. It follows that if in a susceptible individual the number and/or intensity of exposures to a particular metazoan parasite have been great, then the number of parasites will be large, and if a ' pathogenic ' species (or, as a zoologist would term it, ' an ill-adapted ' species) is involved, such infections may result in clinical signs and symptoms. If, on the other hand, the number and intensity of exposures have been minimal, then at no stage of the infection will more than a few parasites be present, and these, even if belonging to a ' pathogenic species,' may not produce any clinical manifestations.

We may summarise these observations by saying that in metazoan infections, although the parasite may be present, the disease with which it is associated may be absent, even in persons who have developed no resistance or immunity (' zoological infection '), a state of affairs which is rarely encountered in infections due to unicellular organisms and which is quite distinct from premunition and from the carrier state.

Amongst the unicellular organisms the parasitic bacteria tend to follow what we may call, for want of a better term, an unplanned cycle of development in the susceptible host and during this period do not individually increase markedly in size or regularly alter their habits, so that their general tendency is to exert similar effects wherever they become localised, although the resulting signs and symptoms will vary according to the particular organ called upon to bear the brunt of the attack. The course of development pursued by the parasitic protozoa appears to be between that just described for the bacteria and that of the parasitic metazoa described in the next section.

The metazoan parasites, once they have gained access to the vertebrate host, tend to follow a very regular cycle of development during the course of which they increase very rapidly in size and undergo considerable changes in their morphology and habits.

In the case of the filarioid nematodes the starting-point of their migration is always the tiny wound made by the arthropod vector ; from this site the anatomical route pursued varies according to the species involved, but individual species are always very limited in their choice of the final habitat. The migratory phase and development of adult forms of those species which parasitise man usually occupy a twelve-month period during which the worms have grown in length from about one millimetre to some five to thirty centimetres, according to the species, and it is not surprising, therefore, that the reactions of the individual to the presence of the different species of filarial worms should vary not only as to the species involved but also with the stage of its development.

It is well known that marked reactions may follow the successful destruction of unicellular parasites, both bacteria and protozoa, by chemotherapeutic measures, but on the whole severe reactions are unusual and are very unlikely to deter the doctor from using a drug which is destructive to the parasite but normally non-toxic to the patient.

The situation as regards the chemotherapy of helminthiasis is, however, very different. The administration of filaricidal drugs to normal persons is followed by no ill effects, but a similar or even smaller dosage administered to patients suffering from filariasis often causes reactions sufficiently unpleasant to induce a proportion of patients to refuse further treatment.

As regards eradication of filariasis by such drugs, evidence is gradually accumulating which suggests that the insect vectors of some species of these nematodes may in certain areas transmit to man strains obtained from local reservoirs other than man. In such areas chemotherapeutic measures cannot prove successful unless they are enhanced by measures directed against the animal reservoir.

Unicellular parasites undergoing development in the invertebrate vector invariably multiply in their insect hosts, but the effects of such multiplication vary greatly according to the species of parasite and the species of the vector. Thus although the malaria parasite causes no harm to its anopheline vector, *Rickettsia prowazeki*, the cause of epidemic typhus, it always destroys the louse which transmits this infection to man ; while the life span of fleas transmitting bubonic plague is often shortened by the multiplication of the causal organism *Pasteurella pestis*.

Multicellular parasites developing in the invertebrate vector may or may not multiply before reaching the infective form. Thus, in the case of all filarial species—the helminth group with which we are primarily concerned—no such multiplication occurs, the microfilariae taken up by the biting fly from the vertebrate host being returned to the same, or another, vertebrate host altered in morphology but not increased in number. On the other hand, in trematode infections the parasites which enter the invertebrate host—in this instance a mollusc—multiply prodigiously before they are released to find another vertebrate host.

These differences in the life-cycles of two groups of helminths must produce different effects on their respective invertebrate hosts, but of these effects we possess very little knowledge, except that in both instances they vary with the species of the parasite and of its invertebrate host.

Earlier in this paper we referred to the fact that the medical practitioner tended

to consider filariasis as a state of parasitisation caused by helminths, some species of which were pathogenic and others non-pathogenic.

More than fifty years ago Van Beneden (1889) wrote: 'A parasite is he whose profession it is to live at the expense of his neighbour and whose only employment consists in taking advantage of him, but prudently so as not to endanger his life. He is a pauper who needs help lest he should die on the public highway but who practises the precept not to kill the fowl in order to get the eggs.' If we still accept this dictum, and I think most of us would, then the non-pathogenic group of filarioid helminths, as represented by *Acanthocheilonema perstans* and *Mansonella ozzardi*, have risen high in what Van Beneden calls 'the profession of parasitism,' for their development in the vertebrate and invertebrate host appears to be unimpeded.

In marked contrast to this group of peaceful invaders, 'this harmonious community of happy worms in happy men' to paraphrase slightly the apt description of Taylor (1938) are the so-called pathogenic filarial species, belonging to the genera *Wuchereria*, *Loa*, and *Onchocerca*. It is true that a proportion of the larval forms, when taken up from the blood, complete their development to the infective stage without endangering the life of the insect, although they may interfere with its activity, and so alter its range of flight as to limit the spread of the disease it carries, but once transferred to the human host the invading parasites tend to cause reactions varying in severity from a mild urticaria to hideous deformities, blindness or even death.

In filariasis, as in all arthropod-transmitted infections, such violent reactions are clearly disadvantageous to both host and parasite and suggest that man is a comparatively new host in the evolution of this group of filarioid nematodes or that another better adapted species of vertebrate host exists. We have some evidence of the existence of such an alternative host in the case of *L. loa*, and this finding, if confirmed, will be of great assistance in understanding the spread of loiasis and will encourage us to search for similar evidence in the case of onchocerciasis and wuchereriasis, diseases which still cause much suffering amongst inhabitants of the tropics.

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The Pathogenicity of 'Entamoeba histolytica'

by

Dr. A. R. D. Adams

Opinion is still sharply divided as to whether *Entamoeba histolytica* is invariably a tissue-inhabiting parasite or whether it can also, or alternatively, live commensally within the large intestine of man. Much has been written by parasitologists and by clinicians in support of the former view. These writers (e.g. Faust, 1941) say that though in some individuals the result of infection with *E. histolytica* is amoebic dysentery with obvious ulceration of the bowel, while in others the infection is not in any way clinically evident, in each and every case there is tissue invasion of the bowel wall by the parasites. In the non-symptomatic cases, they claim, the lesions individually are very small and superficial, and they are very limited in their number and location, so the physical damage caused by the parasites in these non-symptomatic cases is so slight as to cause no gross clinical repercussions from their presence. Lately, other parasitologists (e.g. Hoare, 1950) have questioned the grounds on which these conclusions have been based, and claim that the published data are open to the interpretation that an *Entamoeba histolytica* infection in the bowel of man in many cases may be a purely commensal one; in these instances the parasites live, like *E. coli* and some other amoebic intestinal parasites of man, within the lumen of the bowel and on its mucosal surfaces and do not invade the tissues at all; that is why they cause no physical manifestations of their presence.

On considering these conflicting views it seems to me that what might be called the 'normal' course of the disease,

amoebic dysentery, that is the uncomplicated disease as seen in otherwise healthy well-nourished individuals, lends support to both of them. Notoriously, amoebic dysentery in the otherwise healthy and well-nourished individual is a chronic relapsing disease. There are periods of dysentery recurring irregularly, with intervals of freedom from symptoms which often last for months or years. This suggests that the parasite alternates between pathogenicity and commensalisation in these cases. The disease in debilitated persons suffering from malnutrition and a variety of concurrent infections is another matter; in these cases it is much more acute, severe and continuous.

The question now arises as to what causes the parasite in some persons and at some times to become pathogenic and so to produce clinical manifestations of its presence. Several theories have been advanced to explain the cause of pathogenicity of the parasite. It has been suggested that while some strains of parasites are pathogenic others are non-pathogenic, and there is at present much agreement that strains of *E. histolytica* producing small cysts (under 10μ in diameter) do not cause evidence of pathogenicity in man, and, furthermore, the small cyst-producing amoebae on suitable introduction into animals are non-pathogenic to them also. Beyond this it is inadvisable to go in differentiating non-pathogenic from pathogenic strains of organisms. Though many human infections, indeed most of those in the temperate climates where amoebic dysentery is a rarity though parasitisation is quite common, do not cause evidence of pathogenicity in those infected, nevertheless, on suitable introduction into experimental animals, these strains, most of which produce normal-sized cysts, are found to be as actively pathogenic to the animals.

Yet another view, of French origin, is that damage to the mucosal surface of the bowel by some traumatic agent motivates pathogenicity of the parasite. Lesions in the bowel mucosa caused by any trauma may well facilitate the entry of amoebae into it, and a concurrent infection of any type may lower the resistance of tissues to their invasion.

A further hypothesis, for which there is more substantial support, is that some concurrent bacterial infection or infections are necessary for the establishment and maintenance of the parasites within the intestine as commensals, and that an additional particular type of bacterial infection, probably a specific intestinal pathogen, is essential to the development of invasive lesions (Westphal, 1937). In support of this opinion are the facts that amoebae, so far as I am aware in spite of certain claims to the contrary, have not been established and maintained in bacterium-free culture; furthermore, the treatment of persons with intestinal amoebiasis with certain antibiotics which exert a markedly deleterious action on many forms of bacteria but which are devoid of any obvious effect directly on the amoebae themselves, results in marked modification of the intestinal flora and the simultaneous disappearance of an *E. histolytica* infection from the bowel. Against this are the facts that no constant specifically identifiable bacterial environment has been found essential to the maintenance and continued multiplication of *E. histolytica* either *in vitro* or *in vivo*, and, furthermore, it has often been demonstrated that the tissue lesions due to *E. histolytica* initially are bacteriologically sterile. A characteristic of the histology of all amoebic lesions, wherever they may be, is that there is no inflammatory reaction in or around them; if this does occur it is always terminal and is due to secondary bacterial infection of the lesion.

It is known that certain antibiotics, in particular aureomycin and terramycin, when given orally profoundly alter the constitution of the intestinal flora and that commonly these changes are associated with the disappearance of an intestinal *E. histolytica* infection; but the administration of these same drugs has no effect at all on extra-intestinal amoebic infections. These two antibiotics are freely absorbed from the bowel and they disseminate throughout the body; so their action on bacteria is not confined to those bacteria within the lumen of the intestine. Therefore when amoebae invade tissue the changes in the bacterial flora following treatment with antibiotics have no effect on the tissue-inhabiting amoebae, but they are detrimental only to those amoebae living commensally within the intestine

and presumably in very close association with a varied and numerous bacterial flora. It may well be that the presence of bacteria which break down organic matter into certain assimilable substances or alternatively the presence of bacteria which synthesise certain substances essential to the welfare of *Entamoebae histolytica* is necessary for the maintenance of the parasite commensally or in culture. It can be argued that some such essential substances are present in tissue so the amoebae can flourish there without the need for a concomitant bacterial infection.

The mechanism of the origination of pathogenicity therefore remains undecided beyond the unquestionable fact that it is much more evident in places where bowel trauma, due to other parasites and to the infective bowel conditions, is common than in temperate climates where these are comparatively rare nowadays.

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Physiological Approach to Research Projects in Malaria

by

Dr. W. H. Horner Andrews

Although we have made great strides in the prevention and cure of malaria, we are still largely ignorant of many of the basic pathological processes of the disease. Why, for example, is one individual seriously ill when another, who has no fewer parasites in his blood, is relatively unaffected? Why should the same disease cause renal failure in one case, hepatic damage in another and yet produce no localised changes in the majority of those infected? And, above all, why does a person die of malaria? The aim of the medical physiologist should be to answer these and similar questions.

Malaria is a parasitic disease, and must be considered as an entity. It is, however, convenient to place research into its processes under several headings. The first is into the host and the parasite as individual organisms, which leads naturally to the second heading of the direct effect of the parasite upon the host and vice versa. Thirdly, there is the reaction of one to the other, which is most easily explained by analogy. Arteries possess the power of constriction and their tonus is continually under physiological control. With certain injuries, notably to the elbow region, unphysiological constriction takes place locally, and drastic treatment is usually needed to restore the circulation. It is highly probable that similar, if less immediately spectacular, reactions take place in malaria, and the course of the disease in severe cases becomes easier to understand if it is accepted that local circulatory changes may take place in the viscera.

When I came to work in Liverpool, Professor Maegraith suggested that I should investigate the pathogenesis of centrilobular necrosis in malaria. He postulated that in certain cases of falciparum malaria (as well as in other conditions) there is reflex slowing of the blood-flow through the liver due to constriction of the hepatic veins, which leads to engorgement of the sinusoids and to parenchymal anoxia. A previous hypothesis of his, the renal anoxic syndrome, also is based on local circulatory changes and has been widely accepted. The circulatory change in the liver may be elicited either by liver damage or by general vasomotor action, possibilities which are being investigated.

Very little is known of how the parasite damages the host. No toxin has been found and the metabolic processes of the host, such as the oxyhaemoglobin dissociation curve, so far studied appear to be unaffected. Falciparum malaria can, however, produce lysis of non-parasitised red cells and a similar mechanism may well damage tissue cells.

Once the initial damage occurs the tendency is towards anoxia. Pyrexia increases oxygen utilisation; the parasite itself uses oxygen; destruction of erythrocytes reduces the oxygen-carrying capacity of the blood; in many cases there is a diminished blood supply to the tissues due to a lowered arterial pressure and a smaller cardiac output; haemo-concentration may occur; the pulmonary circulation

may be altered; and there may be sludging of blood, especially in damaged capillaries. Yet there are not necessarily histological changes in the tissues in malaria, although dysfunction of various organs can frequently be shown. When cellular damage does take place it is often most severe in one or two organs only, which supports the hypothesis that some local factor is involved.

Investigation of the problem of applied physiology, namely, that in malaria there may be a change of hepatic blood-flow, has led to research in basic physiology and anatomy, for we found that too little is known of the hepatic circulation for us to start on our main problem.

Our first step was to find whether, as has been reported, the central zone of the liver lobule receives a special blood supply. Our work suggests that this is not so, and that the central zone is the most vulnerable to anoxia because the blood supplying it has already supplied the peripheral and middle zones. We have also found that some arterial branches may by-pass the parenchymal cells and act as shunts.

Our next step was to study the behaviour of the hepatic blood vessels and we have now nearly completed our experiments on isolated livers. The results have been interesting. During the elaboration of our present technique we found that if a liver is deprived of blood for even a few minutes only, its vascular reactions are considerably modified, and a definite resistance to the passage of blood occurs, mainly due to hepatic venous constriction, and the blood tends to stagnate within the sinusoids. Acetylcholine, which previously has been reported not to affect the hepatic veins, produced strong constriction in small amounts in our experiments. This drug, which is rapidly destroyed in blood, acts on the hepatic veins more readily when introduced into the hepatic arterial flow than when introduced into the portal venous. This fact suggests that some hepatic arterial blood may pass through to the hepatic veins more rapidly than portal venous blood, and provides physiological confirmation of arterial shunts. Acetylcholine also opens these shunts, and the net effect of giving the drug is both to divert blood from the sinusoids and, by means of hepatic venous constriction, to slow the blood which still passes down the sinusoids. We have therefore found one mechanism which tends to produce parenchymal anoxia, but its importance in disease has yet to be assessed.

Our next step is to integrate these changes in blood flow with changes occurring elsewhere in the body, and to find how, and under what conditions, a diminution of blood supply to the hepatic parenchymal cells occurs.

Although we have had no success in perfusing human livers obtained at autopsy, we have recently examined the hepatic veins of man and various animals and found that anatomical details, such as the amount and position of smooth muscle in the vessel wall, do not differ vastly in different species. Also, hepatic venous congestion (presumably due to hepatic venous constriction) occurs in all species in shock and many other conditions. Any results, therefore, which we achieve will probably have a wide application.

I hope that I have given you an idea of our approach, in Liverpool, to the problem of investigation of disease. Whilst I and some of my colleagues are working on the reactions of the host, others are more concerned with the parasite itself and how it directly affects the host. Ten or fifteen years hence many of our hypotheses may prove to be wrong, but that is relatively unimportant. It is the approach to the problem which matters, for only by fundamental research in a definite direction can any real advance be made.

Medical Policy and the Demographer

by

Prof. T. H. Davey

Previous speakers this morning have been discussing the results of research into some problems of tropical medicine. For a short time we might turn our attention to one of the problems resulting from the application of modern medical methods in the field of public health in the tropics, namely, the problem of over-population. The present demographic situation is so well known that for our purposes a brief reference to it will be sufficient. The world's inhabitants were estimated in 1950

to total 2,400,000,000 and they are increasing at the rate of 22,000,000 annually, a growth of approximately 1 per cent per annum. This expansion is due to a steadily declining death rate which operates directly by saving lives, and indirectly by increasing the numbers and lengthening the life of the reproductive age groups. Asia, excluding Soviet Russia, contains half the world's population and almost all of its people live by agriculture and are technologically under-developed. Africa has almost 200,000,000 inhabitants and most of Africa also is agricultural and under-developed. The industrial and technologically developed parts of the world, Europe and North America, together hold some 500,000,000 inhabitants, and Russia, also rapidly developing, contains almost 200,000,000. The population, therefore, of those areas which are no longer basically agricultural amounts to almost 700,000,000 : less than one-third of the global population. In these countries rapid population growth is now impossible because the death rate is already so low that it cannot be dramatically reduced and the birth rate approximates to the death rate. The capacity for expansion of populations living in under-developed areas, on the other hand, is very great. Mortality there fluctuates from year to year but is high on average, while the birth rate of primitive communities varies, according to the area, between about 25 and 50 per thousand. The death rate, however, is capable of rapid and dramatic reduction and the imbalance between birth and death rates then results in rapid population expansion.

Populations in under-developed territories began to increase when firm and stable government was established, usually by the colonising powers. Control of inter-tribal warfare and the relief of catastrophies such as famine and flooding were responsible for a decrease in mortality and for steady population expansion. In India, for example, during the fifty years following the first census in 1872 the rate of increase averaged 0·4 per cent per annum, but between 1921 and 1931 it rose to an average of 1 per cent and between 1931 and 1941 it became 1·5 per cent per annum. During the greater part of this period the effect of medical measures was negligible and even during the latter part it was small in comparison with the accelerated expansion which has been demonstrated recently in some areas. The combination of strong central govern ment and modern medical techniques is responsible for the over-population problem in many countries to-day.

It is probably true to say that the recent advances in the treatment and control of tropical diseases have been more spectacular than in other fields of medicine. In some areas where malaria provided the major medical problem, control measures have eradicated it or have made it a disease of minor importance—for example, in Ceylon, Cyprus, British Guiana, Mauritius, Upper Egypt ; in many other territories of Africa, Asia and South America, the local incidence of this disease has been greatly reduced. Again, typhus and louse-borne relapsing fever, when they occur in epidemic form, are rapidly contained and controlled by modern methods of attack on the louse.

In recent years medical services in the tropics have put much more emphasis on prevention, in some territories to a greater extent than in others, and the new techniques, particularly the use of residual insecticides, have been employed by which rapid and efficient control of a number of the major diseases has been brought about. This has introduced a fresh factor and an important one into the demographic situation because a marked reduction in mortality can now be effected with great rapidity. Probably the most striking example to cite is Ceylon because it has had a reliable census since 1897. Birth rates there have remained between 36 and 40 per thousand over the period 1928 to 1950, while death rates have fluctuated considerably owing to malaria epidemics, but in non-epidemic years were about 21 per thousand. Following intensive control by residual insecticides commencing in 1946, the death rate dropped to 14·3 in 1947 and has now fallen to approximately 12 per thousand. The annual population increase before intensive control was about 80,000 per annum and is now of the order of 200,000, a rate of population increase of almost 3 per cent per annum. Similar reductions in mortality have been reported

from other areas where malaria control measures have been successfully applied. No figures are available to demonstrate the effect on population growth of the control of cholera, typhus, louse-borne relapsing fever and plague, diseases which previously occurred in epidemic form with a heavy death rate, but we must assume that the effect has been to cause expansion though to a lesser extent than in the case of malaria because of the more local distribution. These preventive measures have not yet been applied to all the areas in which they could be effective and disease still exerts a stabilising function in the rural areas of most tropical countries. It is western policy, however, to supply the funds, the materials, and in some cases personnel, to spread preventive medical measures to the most remote corners of the earth and we must anticipate that this policy will result in very large population increases. For example, a scheme has been accepted in India for the country-wide control of malaria affecting 200,000,000 people. If population expansion of the order noted in Ceylon is to result, India, which is already suffering from population pressure, will see its living standards sink to a still lower level and grave health and political consequences are likely to ensue.

It would seem to be clear that medical progress in a subsistence economy may relieve disease and at the same time aggravate population pressure, resulting in malnutrition and causing political instability. In under-developed areas stability of the population is maintained by a high death rate balancing the high birth rate ; expansion follows a reduction of the death rate unless fertility also declines. Modern public health techniques cannot be withheld from the vast populations of the tropics on the ground that their use will cause population pressure. The problem with which we are faced is how to use them without producing a crisis. Our aim must be to approach stability at the lower death rate level by a reduction of the birth rate, not necessarily to the same figure as the death rate, but to a rate at which the progress of development will permit population expansion without a drop in living standards and preferably with an improvement.

It will, of course, be noted that the population density of most tropical countries is very much below that of western countries which nevertheless support for their poorest inhabitants a standard of living much higher than that of the average tropical peasant. The under-developed areas could, in fact, be made to support in prosperity a much larger population than that which at the present time is causing pressure and lowering standards of living. The carrying capacity of a territory is directly related to the stage of technological development of its agriculture and economy generally, and to the amount of capital investment in it. Associated with this, capital investment in industry will enable a still larger population to be carried. Population pressure in a territory only means that the exploitation of the natural resources in the form of raw materials, soil, labour and the technical skill of the people must be developed more rapidly. For example, a purely agricultural country dependent on short seasonal rains for its crops cannot support as large a population as if it had a system of perennial irrigation. These resources, however, can only be developed slowly ; the raising of standards of education and agriculture requires years of steady work and the capital investment required for irrigation, communications, building, and the recovery and processing of raw materials is of staggering magnitude. Gunner Myrdal, the economist, has stated that the annual capital requirement of technologically under-developed areas throughout the world is three thousand millions sterling in order to accommodate the population increase and to augment the average income by 2 per cent per annum.

It must be acknowledged that in tropical countries finances and technically trained personnel are insufficient to undertake simultaneously all the desirable development projects. A selection must be made and priorities must be decided after a survey of the proposed activities of different departments and also within each department. It is essential that the whole development plan of the territory should be integrated so that the resources of the country are developed in time and in sufficient degree to absorb the population

increase resulting from public health and other measures. Projects which require development at an early stage are irrigation schemes, improved agriculture, improved communications and markets, and since these require time to bring them to effectiveness they should precede new public health projects unless disease is the primary factor limiting production. Timing and co-ordination are of great importance if the various departmental activities are to be beneficial and not injurious to each other. Integration will also require that each department will co-operate fully, as and when required, in those aspects of the projects of other departments which concern it. Irrigation schemes, for example, will primarily concern engineers, but almost every other government department will have an important function in regard to them—the administration in the siting and planning of new villages, the agricultural department in advising on suitable crops, crop rotation, fertilisers and watering schedules, the health department in the provision of pure water supplies and disposal systems, the veterinary department in livestock introduction or control, and so on.

Wise planning for territories at a subsistence level will necessarily give a high priority to agriculture generally, and particularly to farming as practised by the peasant. The reasons for this are that agriculture is the basic industry of most of the tropics, involving some 80 per cent of the people, and secondly, that the urbanisation and industrialisation which are required to relieve unemployment and to raise living standards cannot take place until *per capita* agricultural productivity has been raised to a level at which each farming family can supply food for itself and for one or more non-farming families, and at which unemployment in agriculture can supply the labour needs of industry. Before agricultural productivity can be markedly raised many local customs must be changed and many problems solved, such as conservation of soil fertility, water management, land tenure, plant pests and diseases. History has shown that a rise in standards of living improves health also, and indeed in civilised countries many communicable diseases have almost disappeared without any medical attack on them because better food, housing, clothing, and so on, have lessened transmission to a point at which their incidence was negligible. The activities of departments other than the medical may, therefore, be expected to reduce the burden of disease on the community.

Secondly, within the health department, priority should be given to the control of those diseases which most markedly affect production. These include the crippling diseases such as yaws, onchocercal blindness, and also diseases occurring in epidemic form at times of seasonal activity such as planting or harvesting. A strictly logical analysis would force one to the view that rapidly killing diseases were of lesser importance from the community's viewpoint than crippling diseases. Other considerations, however, affect the issue, and though priority should be given to crippling diseases, in the long-term interest of society mortal endemic diseases must also be attacked as well as the epidemic diseases which seriously disorganise the community.

A third way in which the ill effects of the new health techniques can be avoided is to leave the operation of the techniques to the people themselves after a suitable period of training. Commonly these control measures are operated very efficiently by local personnel under the supervision of skilled technologists from western countries. After a period of training, responsibility for the organisation and operation should be handed over to the trainees, who will often not demand or reach the high standards of western countries. Disease control without the active co-operation of the inhabitants, though it may be efficient and of great benefit, has little educational value, whereas the acceptance of full responsibility for the campaign by the people themselves gives experience of administration and permits them to work out their own standards.

Finally, I must refer to family planning. The elevation of living standards appears to be associated with a reduction in birth rates after a lag of some decades, but the only immediate and direct means of lowering fertility is by deliberate family limitation. This is in many places rejected on social or religious grounds and it must

be realised that it will only become a factor in reducing fertility when the bulk of the people desire it. The wish to limit the family will develop when economic pressure arises as a result of a demand for material things and when education convinces the people that a small number of healthy children on a good standard of living is better than a large number on a low standard. Family planning will spread very slowly in under-developed areas and its adoption will necessitate a change of values which will probably only follow economic and educational betterment. Before it can significantly reduce the birth rate considerable population growth must be expected.

It is highly improbable that the bulk of the capital necessary for the comprehensive development schemes will be obtainable from outside the under-developed territories owing to the lack of security consequent upon political instability and the intense nationalism of these rapidly developing communities, and also owing to rearmament and other operations requiring huge expenditure in the countries possessing investment funds. A considerable proportion of the capital required for development must come from within the under-developed territory and it is available there in large amount in the form of labour. Self-help schemes at the village level can develop national resources with little expenditure and they can materially raise standards of living and education. The value of such schemes has been demonstrated in Mexico by UNESCO and in Hindustan by the Indian Government. Such projects require to be multiplied greatly so that the whole population is taking an active share in the development programme. Health education is an integral part of these projects, its aim being to change the practices which encourage dissemination of disease, and so to relieve the medical service of the load of illness resulting from pollution and insanitary habits.

Capital from outside the territory will, however, be needed for large schemes and with it technical experts to develop resources in the form of raw materials, minerals, and for irrigation and hydro-electric schemes. In a balanced development plan, however, such projects and subsequent industrialisation will not be allowed to deplete the labour required for producing sufficient food. Where possible, capital investment should first be directed to improving agricultural productivity, as, for example, by providing irrigation for food-producing areas.

The arguments advanced here may be summarised as follows. Unintegrated medical advances in many tropical areas are rapidly producing a demographic and thus a political situation which is dangerous to the stability of the territory involved. As has been stated by the Malaria Control Officer in Mauritius : ' It is not very satisfactory to introduce a grave problem hand in hand with a beneficial public health scheme, and it is emphasised that steps should be taken to ensure that the inevitable population increase can be housed, fed and employed before proceeding with similar malaria control schemes elsewhere.' Sustained progress in under-developed territories is best assured by providing an integrated and phased plan of development involving the wise use of labour, capital investment, education and, above all, agriculture, because it is the basic industry, and because low *per capita* agricultural productivity at present limits urbanisation and industrialisation which are needed to solve the problem of land pressure and unemployment. Integration of development schemes and their proper balance and timing are essential if modern public health advances are to bring benefit to the population of under-developed areas. This is acknowledged by the Technical Assistance Board of the World Health Organisation, who have accepted, amongst other criteria, that the control of major communicable diseases should be directly related to improving the health of the population for productive purposes.

Observations on the Infectivity of Smallpox

by

Prof. A. W. Downie, Dr. K. R. Dumbell and Dr. K. McCarthy

Smallpox is a severe disease. It is infectious and it is dangerous to life. In the

eighteenth century smallpox was widespread in this country and ranked as one of the major causes of death. To quote but one example : in Liverpool from 1772 to 1774 of every eleven people buried two had died of smallpox. Today things are very different. Smallpox is no longer a common disease in this country. In fact, cases only occur when the infection is brought into the country from parts of the world where smallpox is still prevalent ; and the outbreaks which do occur are small in extent though often serious in nature. We propose to show how certain features in the natural history of smallpox enable this degree of control to be maintained while other virus diseases like influenza, measles, mumps and chickenpox appear to spread unchecked.

Not all cases of smallpox are severe. Most people who travel abroad through areas where smallpox is frequent have been vaccinated. If they get infected at all they suffer the mildest form of the disease and we have shown the presence of the smallpox virus in people who on the whole surface of their body have had only one typical spot. A man can arrive in this country without any illness being suspected. All remains well for some ten to fourteen days and then severe cases of smallpox may appear, apparently out of the blue, among the family and close contacts of this man, who, now that he is suspected and carefully examined is found to have only one or two smallpox spots.

Smallpox is caused by a virus—an extremely minute living organism which is present in myriads in the skin rash and in similar spots that occur in the mucous surfaces of the throat and nose. We believe that most of the virus-carrying specks which transfer smallpox from one person to another come, in fact, from inflamed spots in the throat or nose, and are shed by the smallpox case in a fine, invisible spray as he talks, breathes or coughs. One or more of these droplets may be taken in by a susceptible person in whom the smallpox virus can grow. This person may develop anything from severe to mild smallpox, or, if he is protected by a recent vaccination, anything from mild smallpox to nothing at all. Even in the man who will develop a severe disease, the virus does not at first produce any obvious effect. We believe that the infecting virus remains at its site of entry growing within cells close by and gathering its forces. After an interval of many days this tiny local focus breaks into the blood stream and multitudes of virus particles get distributed throughout the tissues of the body. At this point, usually twelve days after infection, the new patient begins to feel ill. Whenever a virus particle is filtered out in the capillary blood vessels of the skin and throat a small focus is started which after a couple of days becomes a blister and develops into a typical smallpox spot. Now this new patient is, in his turn, shedding from his person large numbers of virus-carrying particles. Now, and not until now, he is infectious to others.

Admittedly a part of the above is somewhat speculative. The whole picture is built up from small details of evidence and some parts of the story are yet unproven. In particular the site of the first entry of virus has so far escaped direct observation. We, in Liverpool, have shown that the smallpox virus may be found in the blood at the time when the patient first starts to feel ill, but not before this ; that is, at the time when we presume the initial focus at the site of entry to have burst into the blood stream and the virus to be on its way to the skin. The rash is not due to appear for another two to three days and virus cannot be found at this stage on the surface of the skin or throat. We have only been able to demonstrate the presence of virus in the skin and throat after the rash has appeared. These findings fit in well with the story of smallpox epidemics of the past, the evidence from which indicates that a smallpox patient is not infectious during his incubation period.

We should at this point give some idea of how, in the laboratory, the presence of smallpox virus is demonstrated. This virus can be made to infect the lining membrane of a fertile hen's egg some ten days before the chicken is due to hatch. Two days later, if a little of the shell is chipped away, infection is shown by the appearance of small white spots on the surface of the membrane. Other viruses can be grown in this way, but the spots produced by smallpox virus can be distinguished from those produced by other viruses.

To control an outbreak of smallpox there are three essential things to be done : any case of smallpox must be rapidly recognised and diagnosed—with laboratory confirmation in doubtful cases ; everyone with smallpox must be nursed and treated in strict isolation-quarters ; any person who is known to have been with someone who develops smallpox must be found, offered the protection of vaccination and watched daily. Further to this last point, an effort must be made to find the contacts of those who had contact with the first case. If these measures are instituted promptly it is possible to get one step ahead of the spread of the virus and halt the outbreak there.

The sort of outbreak that is likely to occur is well illustrated by what happened in Glasgow in 1950. An Asian seaman arriving at that port was taken to hospital with a slight fever. A few days later he developed a mild skin rash and was transferred to the isolation ward as a case of chickenpox. Twelve days after this, cases of smallpox started to occur among the patients and staff of the two wards in which he had been nursed. Over the next few days seventeen people went down with smallpox. The hospital was immediately closed to visitors. All known contacts of those affected were sought out and offered vaccination. The help of local papers and of the B.B.C. was enlisted to appeal widely for any other contacts to report to their local Public Health authority. Also a special vaccination centre was opened in Glasgow which was available to anyone desirous of renewing their protection. So prompt and efficient were these measures that the disease spread no further than to those in immediate contact with the first case.

Although vaccination is an efficient protection against smallpox for the individual, control of an outbreak is possible in this country to-day largely because the patient is not infectious to others during his incubation period of ten to fourteen days, so that there is generally time to offer the protection of vaccination to those of his contacts who have not availed themselves of it in advance.

Imagine, by way of contrast, a virus disease with a much shorter incubation period where a person becomes ill only three to five days after being infected. Imagine further that before he becomes ill, say only two days after infection day, he is already infectious to others. Then by the fifth day when he feels ill and shows the signs of his disease he will have been infectious for two to three days and the people he has infected will already themselves be infecting other people. A disease, familiar to everyone, which behaves in this way is influenza. To control a disease which spreads at this rate it would be necessary to prepare in advance—perhaps by immunising a large portion of the population. Influenza defeats us here because the virus which causes it undergoes variation so that immunisation protective against the virus of one epidemic might be unavailing against the slightly different virus which caused the next outbreak. This is why the use of influenza vaccine has at some times been successful and at others quite useless. It is possible that after some years we may find that we have met the full range of variation of influenza and can give a comprehensive protection ; on the other hand new variants may be produced continually.

Rapidity of spread is not the only way in which infectious disease slips out of control. One of the most important spreaders of smallpox is the man with a very mild disease. This idea can be carried a step further. In some diseases a virus may infect a person, grow in them to the stage of making them infectious to others, but not go that further stage and make them ill. This can sometimes happen with influenza. These transmitters of disease who do not themselves become ill are called, appropriately enough, ' infective carriers ' of the disease in question. There are such carriers in many diseases but in one in particular the carrier state is the most usual sequel to infection and in only occasional instances is infection followed by serious illness. This disease is, of course, poliomyelitis. In a poliomyelitis outbreak odd cases occur here and there throughout a town with no apparent connection between them. All sorts of theories have been advanced to explain this, but when reliable laboratory methods of detecting the virus were found someone had the idea of testing a representative cross-section of the whole community for the presence of

the virus. Then they found the answer. During an outbreak large numbers of people harbour the polio virus, allow it to multiply and pass it on. Only a small proportion of these develop the true disease with paralysis. The virus may be just as widely and rapidly spread as influenza, but its spread is, in a sense, underground and only here and there does harm result. Those trying to find ways of controlling this infection have got a difficult problem.

Smallpox is in many ways an exception to the general run of virus diseases. There is, as far as we know, no carrier state. Anyone who is capable of passing on the infection has at least one potentially recognisable 'spot' to give him away. Also, smallpox is not infective during its incubation period and finally we have, in vaccination, an adequate means with which to protect against the disease. In most other virus diseases there are people who can pass on infection before they become ill, or indeed, without becoming ill at all.

In smallpox, because of its special features, we are generally able to stop an outbreak of the disease from spreading even when the community has not fully protected itself in advance. In some other virus diseases, because the spread of disease in the community is too rapid, or too unpredictable, no control measure introduced during an outbreak is likely to achieve much success. Attempts at control must be directed towards prevention and plans must be laid well in advance. So much for the present position. What hope does the future hold?

Looking at the problem in a purely speculative fashion there seem to us to be three possible approaches. We could perhaps exterminate a virus in this country so that, as long as it was not reintroduced we would be free of that disease. Even if extermination were possible, however, it would probably not be practicable to prevent the virus being brought again into the country and if it were a rapid spreader all the work of extermination would have gone for nothing.

Secondly, we might concentrate on inducing a state of immunity to a particular virus. This approach seems to hold out the best chance of practical success. There are several virus diseases against which a protective vaccination is available and soon the list may be increased. It is also possible that a chemical prophylactic against virus infections may be found—as has been done successfully against malaria.

Thirdly and lastly, we might perhaps so modify a virus that although it still *spread* throughout the community with undiminished vigour, its power to cause harm had been removed. This possibility is unlikely at present, but it is a fascinating speculation for those pursuing science for its own sake.

THE STRUCTURE OF NORTH AND CENTRAL WALES

At the meeting of the Association in Liverpool there was a discussion in Section C (Geology) on the morning of Tuesday, September 8, on the above topic. It was opened by Prof. O. T. Jones, F.R.S., of Cambridge, who reviewed the probable conditions of stress under which various geological structures were developed.

Prof. R. M. Shackleton, University of Liverpool, followed with an account of recent investigations on the older rocks of Anglesey and there was a brief note from Sir Edward Bailey, Edinburgh, who was unfortunately unable to attend, on some relations of the Mona Complex in the Lleyn Peninsula.

Dr. D. A. Bassett, University of Glasgow, reviewed the development of cleavage in relation to folding in the Talerddig area of West Montgomeryshire. Finally Dr. D. A. Powell, University of Birmingham, gave the results of a recent gravimeter survey in the area.

The Trends of Geological Structures in Relation to Directions of Maximum Compression

by

Prof. O. T. Jones, F.R.S.

Introduction

Certain elements of geological structure such as folds, faults of various kinds and cleavage are often found in association. It has long been customary to regard folds as a result of compression acting in a more or less horizontal direction in the earth's crust and comparable structures have been produced experimentally by the application of horizontal compressive forces. A fold has an axial region, crest or trough and limbs or flanks ; in some folds the crest or trough may have a strong inclination, called the pitch, in the direction of the axis. In them the trend of the limbs may diverge widely from that of the axis. It is obvious that in a number of pitching folds with varying dips in the flanks the only direction which is common to them all is the trend of the axis, and it may be expected that this trend is normal to the direction of compression under which the rocks were thrown into folds.

Navier about 130 years ago investigated the probable conditions of stress in a material which lead to fracture and Dr. E. M. Anderson subsequently applied his theory to various types of faults.

A rock in the earth is subjected to complicated stresses which can be resolved into three normal stresses acting on planes mutually at right angles, and shearing stresses acting parallel to these planes. A different set of planes can be chosen such that there is no shear along them. The complicated stress then becomes equivalent to three normal stresses only ; such planes are called principal planes and the normal stresses, principal stresses.

According to Navier's theory fracture takes place when the shearing force on a particular plane exceeds the strength of the material.

A cube of material in a testing machine is under normal pressure applied to the top and bottom faces. The maximum shear stress occurs on diagonal planes at 45° passing through the edges of the cube.

It has been found in practice however that fractures make a smaller angle with the direction of the applied pressure. This can be explained if account is also taken of the component of the applied pressure which acts at right angles to a possible plane of fracture. The material thus ruptures on a plane on which the shearing stress is somewhat less than the theoretical maximum, but on which the normal stress is a good deal less than on the plane of maximum shear.

A piece of rock in the earth is subjected to the weight of the material above it and is confined on all sides so that in general it is in a state of complicated stress. If however, this stress can be replaced by principal stresses then Navier's rule states that if these are arranged in order of magnitude the plane of fracture includes the stress of intermediate value and the normal to the plane includes the greatest and the least principal stresses.

In geology one attempts to infer the directions of principal stresses at the time when various structures, particularly faults, were developed.

If we call the principal stresses acting on a rock in the crust H, V and L, V due mainly to gravity is usually vertical, or nearly so, H is a horizontal compression and L a confining pressure which is usually a fraction of H. Since, except in the roof of a hole in the ground or a ceiling, V is a compressive force, we are dealing in most cases with three compressive stresses of unequal magnitudes. According to Navier's rule if H>L>V the fracture or fault trends at right angles to the maximum compression and its plane is inclined at about 35° or somewhat less to the horizontal, as in strike faults.

If H>V>L the fracture is in a vertical plane which makes the same angle with the direction of maximum compression. Such are the characteristics of tear or transcurrent faults. If, however, V>H>L the fracture trends in the direction of maximum horizontal compression and makes the above angle with the vertical as in the majority of normal faults.

It is theoretically possible for two sets of fractures complementary to one another to develop under each of the above conditions, but owing to some local variation such as greater ease of movement in a particular direction one set may predominate over the other.

As noted above the direction of maximum horizontal compression can be deduced from the trend of the axes of folds. When the so-called strike faults develop they must in general conform to the axial trend rather than to the local strike ; thus these faults are not happily named.

One may infer from general considerations that when cleavage is developed its strike is more likely to conform with the axial trend than with any other direction.

In what follows the relations between the trends of fold axes, faults and cleavage are examined for certain areas which have been surveyed in detail in recent years. These are (1) the Ordovician area between Builth and Llandrindod, (2) the adjoining Silurian area, (3) the Plynlimon and Llanidloes districts, (4) The Merioneth dome, (5) the Berwyn dome and the Llangollen syncline and (6) parts of Denbighshire.

1. *The Ordovician area between Builth and Llandrindod.*—The Ordovician is overlaid unconformably by the Silurian or is faulted against it. Although the rocks consist largely of shales there is no cleavage.

Folds occur only in the central part of the area; the trend of the fold axes is 25 ± 3. [It may be noted that all directions are measured in degrees east of north and the trends are averages weighted where possible according to the length of the structure with a particular trend. The averages and standard deviations are given to the nearest whole number.]

Strike faults in the same area trend 33 ± 9, and thus depart by 8° from the direction of the fold axes. If, however, all the strike faults in the area are considered together their trend is 26 ± 12. The district is traversed by many powerful tear faults. These have a trend which is oblique to the folds and strike faults. They fall into two groups one trending 103 ± 3 and the other 54 ± 7; the angle included between them is 51°, and the direction of the bisectrix is 128, which differs from the normal to the fold axes (115) by 13° and from the normal to all the strike faults (116)

by 12°. In spite of these departures it may probably be assumed that the direction of maximum compression during the period of movement was not far from the mean of these trends or 120°.

2. *The Silurian area of the Builth and Llandrindod district.*—These rocks are only moderately folded but are traversed by powerful faults. The fold axes trend 40 ± 9 and the strike faults at 33 ± 8; their normals are thus 130 and 123. These are so little different from the direction of maximum stress in the Ordovician area that taken by themselves it might be argued that the Silurian and Ordovician rocks had been affected by movements at the same period.

Not only however is there a great unconformity at the base of the Silurian (Upper Llandovery) but the great tear faults which traverse the Ordovician do not affect the base line.

Some of the strike faults are displaced by tear faults which are obviously of later date and may possibly belong to a system like the parallel Church Stretton disturbance which are post-Carboniferous.

3. *The Plynlimon and Llanidloes districts.*—These afford the most perfect examples of pitching folds to be found in Wales. The pitch is northward in the north and southward in the south at about 10° to 12°. Along the major uplifts the upper part of the Ordovician is brought to the surface. The trend of the fold axes is 19 ± 4 and the presumed direction of maximum compression is therefore 109. There are comparatively few strike faults but they hug the fold axes so closely that their trend is obviously the same. There are however many oblique faults which belong to at least three different sets. One of these is the Llyfnant fault in the north-west of the Plynlimon area; it has been proved to be a tear fault of considerable horizontal displacement. Its average trend is 85 ± 4 and thus makes an angle of 24° with the presumed direction of maximum compression. This is not very different from the angle of 25° to 35° which a tear fault would make with that direction.

The majority of the faults belong to the set of mineral veins of lead-and-zinc and occasionally copper-ores. The trend of these is 67 ± 11, and thus makes an angle of 42° with the presumed direction of maximum compression. With a little stretching these might be brought within the limits of variation of tear faults. They are however known from numerous mine workings to be normal faults with an inclination usually between 45° and 70°. If they are normal faults of the same period of earth movement as the folds, strike faults and Llyfnant tear fault their trend should be 109°. Either therefore Navier's rule fails completely to account for these veins or they must be attributed to a different and obviously later period of movement. For what seemed good and sufficient reasons I attributed them to the Caledonian movements which folded the rocks. Finlayson however regarded the mineralisation as post-Carboniferous. The evidence of the trend of the mineral veins clearly supports his opinion.

In the south part of the Plynlimon district the Ystwyth fault behaves as a normal downthrow, to the north, of about 3,000 ft. It truncates the heavily mineralized lodes of Cwmystwyth mine and great masses of lead ore are incorporated in the fault material. Its average trend of 82 ± 6 is different from that of the mineral veins and confirms the evidence of its relation to them that it belongs to a later period.

4. *The Merioneth dome.*—An anticlinal axis near the centre of the dome brings the lowest of the exposed Cambrian rocks to the surface for a length of 6 miles and a maximum width of about 2 miles; there are a few less well-defined folds in other parts of the area. The fluctuating courses of the fold axes make it difficult to define an average trend, but it appears to be about 8, and one may therefore assume that the direction of maximum horizontal stress was about 98.

There are a great many faults of a total mapped length of over 100 miles. A histogram of fault directions shows that the most favoured directions are (a) between 20° and 40°, (b) around 60; and (c) 140° to 170°; there are scarcely any in the direction of maximum stress as inferred from the folding.

Taken separately, the trends of these

groups of faults are (a) 21 ± 9, (b) 57 ± 10, and (c) 153 ± 11. The majority of the first group occur on the west side, while most of the others occur on the east and south-east side though a few of both trends occur in the central part of the area. To the third group belongs the most powerful and longest fault in the area which behaves as a downthrow to the east. The second group (b) which trend nearly at right angles to its trend may probably be regarded as fractures subsidiary to this great fault.

It is impossible on Navier's Theory to account for these two groups by the stress conditions inferred from the fold axes. Either therefore this theory fails in this area, or these faults were developed at a later time when the stress conditions were radically different. One may probably assume that the maximum horizontal stress was then normal to the great eastern downthrow or in a direction 63°. That fault group should in that case be regarded as strike faults of that period, and those of the second group (b) as normal faults. The difference between their average trend of 57° and the trend of the above normal to the third group (c) is not significant in view of the somewhat large standard deviations.

In view of the close correspondence between the inferred direction of maximum stress during the faulting of the Merioneth dome and that which was assumed to have occurred during the production of the lode faults of Cardiganshire and Montgomeryshire it is tempting to assume that the Merionethshire faults were developed in post-Carboniferous times. It would be interesting to know whether the gold mineralisation of that area is related to the (b) group of faults.

The western group of faults (a) make an angle of about 40° at least with the inferred direction of maximum stress during the later movement. It is therefore improbable that they can be tear faults of that movement since their trend departs by rather more than the standard deviation from that of the fold axes, but in view of the difficulty of determining the latter satisfactorily the difference may not be significant. They are most probably to be regarded as strike faults of the earlier period of movement.

5. *The Berwyn dome and the Llangollen syncline.*—These two areas, one mainly of Ordovician the other of Silurian rocks, may be regarded as complementary structures. The east-west trend of the Llangollen syncline is remarkable in the Lower Palaeozoic rocks of Wales. The western flank of the Berwyn dome has the N.N.E. trend of most of North and Central Wales, but its northern flank is parallel to the syncline. In the south part of the dome some folds range N.E.-S.W., but there are some structures with a more nearly E.-W. trend.

South of the Bala fault the base of the Silurian on both flanks of the syncline trends at 98 ± 5, which may probably be regarded as defining approximately the general axis of the syncline. Two major faults trend at 103 ± 8, which nearly agrees with the probable direction of the fold axis. The normal to these two structures is 8 and 13 ; the difference between them is not significant and one may infer that the direction of the maximum horizontal stress during the folding and strike faulting was about 10°.

There are several oblique faults which trend at 157 ± 10 ; or at an angle of 33° with the presumed direction of horizontal compression. These can therefore be regarded as tear faults related to the folding and strike-faulting. On the other hand, they may be the result of a later period of movement, directed at about 67°. In the former case their fracture planes should be vertical and in the second case inclined.

The Silurian rocks are in general strongly cleaved ; its average strike at 23 localities recorded on the one-inch geological map is 96 ± 12. Whether therefore one relied on the fold axes, the strike faults or the cleavage strikes the direction that would be inferred for the maximum horizontal stress would be almost identical.

The strike faults in the northern and central Berwyn dome trend at 97 ± 5 which again is close to the trends of the syncline to the north.

Cleavage is recorded at a few places only ; its trend is 80 ± 16. The agreement with the trend of the strike faults is not good ; on the other hand, the scatter is large, and the difference may not be significant.

On the south flank of the Berwyn dome near Llanfyllin an anticline with a N.E.-S.W. trend is crossed by corrugations with a more nearly E.-W. trend. The direction of stress in the area as a whole seems to be that implied by the trend of these corrugations rather than by that of the anticline on which they are superposed (cf. Denbighshire below).

6. *Denbighshire.*—The generalised structure of the Silurian rocks of Denbighshire is that of a north-easterly pitching syncline, the strike on the west flank being nearly north and south and on the south flank nearly east and west. The rocks near the western margin are, however, traversed by many minor folds or corrugations which are superposed on the general trend; their direction is 92 ± 3. A number of faults in the same area have the same direction.

The area is traversed by many powerful faults which can be proved to be tear-faults; the majority of these trend at 160 ± 7, the remainder at 34 ± 8. They include an angle of 54° which is a reasonable angle to expect among complementary sets of tear-faults. The bisector of these trends is 7°, which may be assumed to be the direction of maximum horizontal stress. There is, therefore, close agreement between the direction of greatest principal stress inferred from the minor folds and 'strike faults' (2°) and that from the tear faults (7°).

According to Prof. Boswell the cleavage strike is remarkably constant lying between extremes of 70 and 110 but is usually a little north of east. The direction of stress given by the cleavage, so far as can be judged, is, therefore, in agreement with that inferred from other criteria, and supports the assumption that the minor corrugations which are the latest expression of the stresses gives a better approximation to the trend of the stresses than does the generalised structure.

The faults trending at 160° have nearly the same bearing as others in Cardiganshire and the Merioneth dome which have been suggested to be of post-Carboniferous age. The relation of the Denbighshire Silurian to the Carboniferous proves however that these are of pre-Carboniferous age.

The above general survey of the structures in North and Central Wales confirms in general the relation between major structures due to compression envisaged in Navier's theory.

The Structure and Succession of Anglesey and the Lleyn Peninsula

by

Prof. R. M. Shackleton

The stratigraphy of North Wales may be said to begin in Anglesey and Lleyn. The pre-Cambrian rocks exposed there constitute the only visible evidence concerning the first two thousand million years or so of the evolution of the region. Our knowledge of these old metamorphic rocks, which are known as the Mona Complex, is mainly due to Edward Greenly.[1] According to his view the Mona Complex belongs to two systems. The older, consisting of gneisses ('The Gneisses') was compared to the Scottish Lewisian. The gneisses were believed to represent an ancient floor of rocks which had been intensely folded and metamorphosed within an ancient mountain chain. The younger system (the 'Bedded Succession') consists of metamorphosed sediments and volcanics. It was supposed to rest unconformably on the gneisses and to owe its metamorphism to a second pre-Cambrian orogeny which resulted in a series of vast recumbent folds. The rocks were thus believed to be upside down over much of Anglesey.

I believe that most of this interpretation (which Greenly advanced with clear warnings that it must only be regarded as a working hypothesis) is wrong. Since Greenly's day methods have been developed for distinguishing top from bottom in metamorphic rocks which may be applied in the Mona Complex. It then appears that Greenly's succession in the schists was upside down; the Holyhead quartzite is near the base and the Fydlyn tuffs at the top of the visible succession. Graded bedding can be seen particularly well in the South Stack Series; it can also be seen close to several of the undisturbed gradational contacts between sub-divi-

[1] Mem. Geol. Surv. *The Geology of Anglesey*, Vols. I and II. 1919.

sions of the Bedded Succession. Greenly deduced the sequence from the evidence of contained fragments, which is a notoriously difficult and untrustworthy method. The most distinctive fragments were of red jasper, identified with those in the Gwna group. These jasper fragments must, I suppose, have been derived from rocks older than any now exposed.

Reversal of the order in the Bedded Succession immediately raises doubts about the position of the gneisses. From their position as the 'Old floor' they would seem to have been removed to the 'Old roof,' for Greenly claimed that they occur next to the Fydlyn end of the Bedded Succession. It is also suggestive that the gneisses contain a marble-graphite schist-quartzite association, which is strikingly similar to the less metamorphosed associations in the 'Penmynydd zone' and in the Gwna group of the Bedded Succession. But it is in Lleyn that the improbability of the 'Old floor' is most apparent. Sir Edward Bailey and I have been studying the pre-Cambrian rocks there, and their relation to the Ordovician. The most important published work on the Lleyn pre-Cambrian is that of Matley,[1] but it may fairly be said that much of Matley's interpretation was taken from Greenly. On his own evidence I do not think that Matley could ever have arrived at the conclusion that the Lleyn gneisses, which as he and Greenly recognised, are quite remarkably similar to those of Anglesey, are separated by an unconformity from the schists. These gneisses outcrop at intervals for thirteen miles along the strike, from Nevin to near Aberdaron. The grade of metamorphism in the adjacent schists wanes rapidly away from the contact, yet this contact was supposed to be an unconformity. No explanation was suggested for the distribution of the metamorphism and no unconformable contact was seen. On the contrary, we consider that in several clear sections we can demonstrate a gradation from the gneisses into the schists. There is thus no unconformity and all the rocks belong to one system. The gneisses, often with sillimanite, represent the most highly metamorphosed part of the Mona Complex. They are migmatites formed from the rocks of the Bedded Succession as a result of the introduction of granitic material.

The structures of the Mona Complex are also, I believe, very different from those postulated by Greenly. The vast recumbent folds which suggested an ancient mountain chain with the anatomy of the Alps do not seem to exist; they were inferred from relationships in Holy Island. There, three main blocks are separated by faults; the present surface exposes rocks which, before the faulting, were at three different levels in these three blocks By combining the evidence from the blocks, a deep section was constructed. Greenly thought that in some places New Harbour Beds were underneath the South Stack Beds and the Holyhead Quartzite on top, while in other places the opposite structural succession was seen, and his section accordingly showed great recumbent folds. The succession appears to me, however, to be upward from the Holyhead quartzite in all cases, except that there are more beds of South Stack type stratigraphically below the Holyhead Quartzite. Moreover, wherever I can find way-up evidence in small folds, these folds are the right way up: they all face upwards. Therefore I regard the rocks of Holy Island as arranged in a normal succession, in which the rocks, apart from the slight overturning in steep isoclinal folds, are the right way up.

Another outstanding problem in the Mona Complex is the mélange. This is a gigantic breccia. In Lleyn, where it is well exposed, Bailey has studied it and he is satisfied, as were Greenly and Matley, that it is tectonic, that is to say the result of deformation of the earth's crust, rather than the consequence of submarine sliding. However the distinction between tectonic and non-tectonic appears rather indefinite nowadays. Not all the mélanges in Anglesey are of one kind; undoubted submarine slide-breccias are, for instance, to be seen interbedded in Ordovician black shales at Porth Padrig on the north coast.

Bailey and I have also re-examined the relations of the Ordovician to the Mona Complex in Lleyn. Matley considered that near Aberdaron, the Ordovician was

[1] Matley, C. A. (1928). "The Pre-Cambrian Complex and Associated Rocks of South-western Lleyn." *Quart. Journ. Geol. Soc.*, **84**, 440.

exposed in a window opened by the erosion of an overthrust mass of Mona Complex. We find on the contrary that the Aberdaron Ordovician rocks occupy a faulted syncline and rest unconformably on the Complex. Very oddly, they seem generally to rest (not only near Aberdaron) on the gneisses rather than on other members of the Complex. One might suppose that when the Ordovician sea advanced, the gneisses lay nearly flat at the surface with schists beneath them. Possibly however this apposition of gneisses and Ordovician is fortuitous.

Regarding the Caledonian structure of North Wales in general, I would emphasise two aspects. First, the change in character of Caledonian structures from Anglesey to Shropshire, and second the change in trend of the structures from Merionethshire to Denbighshire. These two aspects correspond to cross section and plan. In Anglesey, thrusts dominate the Caledonian structures. The pre-Cambrian basement is sliced through and through. In Carnarvonshire the Cambrian and Ordovician are intensely compressed and cleaved. Towards Shropshire the cleavage and folding gradually die away. These differences express the different structural conditions met in passing away from the interior of a mountain chain towards the foreland. The salient feature of the pattern of Caledonian structures in North Wales is the change in fold trend. This is clearly shown by a map of cleavage strikes.[1] It is often supposed that the Caledonian structures trend through North Wales in a south-west - north-east direction, but this is a misleading simplification. There are large areas, especially in Merionethshire, where the structures trend north-south, and others, especially in Denbighshire, where the trend is east-west. The famous in particular for two features. One is an abundance of pillow lavas, with two trends are none the less part of one system. In places there is a gradual swing, in others a sharper though continuous curve from one trend to the other. The swing is most pronounced in a zone extending south-east from Anglesey to Shropshire. This zone itself appears to be shifted sinistrally, possibly as much as eight miles, by the Bala fault. More detailed structural analysis, especially in the Bala district, is needed to evaluate the shift precisely.

[1] To be published in *The Liverpool and Manchester Geological Journal*, 1 part 3, in press.

The Mona Complex in Lleyn and its Relation to the Ordovician

by

Sir Edward Bailey, F.R.S.

The most ancient rocks of the Lleyn Peninsula occur as a strip along the north-west coast from Bardsey Island to Nevin, and are an isolated part of the Mona Complex of Anglesey. It has been suggested that they have been thrust over associated Ordovician strata that make the main part of the peninsula ; but recent work has shown that they are unconformably overlain by the latter. They are famous in particular for two features. One is an abundance of pillow lavas, with variolitic developments comparable with those found among the much later pillow lavas of the Alps. Another is the exceptionally broken condition of much of their material, due to earth-movement of pre-Ordovician date. Some of the complex consists of coarse igneous intrusions with accompanying metamorphic schists. These crystalline rocks tend to outcrop alongside the Ordovician cover, which suggests that they may occupy an upper position in the complex as a whole.

The Folds and Cleavage of the Talerddig District of West Montgomeryshire

by

Dr. D. A. Bassett

The following account is based on a detailed study of the stratigraphy and structure of an area which lies astride the Central Wales Syncline between the villages of Talerddig, Montgomeryshire, and Mallwyd, Merionethshire.

The rocks of the area are predominantly greywackes and graptolitic shale which can be subdivided into four lithological groups, which are, in upward succession, the Gelli Shale Group (*Monograptus crispus* zone), the Talerddig Grit and Shale Group

Monograptus griestoniensis zone), the Dolgau Mudstone Group *Monograptus crenulatus* zone) and the Denbighshire Grit and Shale Group (*Cyrtograptus murchisoni* and higher zones). Each group can be subdivided into two or more easily distinguishable members.

There is marked variation in thickness of almost every member of every group, but generally the beds thicken northwards as opposed to the prevalent attenuation northwards in the districts to the south. The most important marker horizons from a structural standpoint are the Grit Member near the middle of the Talerddig Group, the Blue Shale Member at the base of the Dolgau Group and the various grit bands in the Denbighshire Group.

Exposures are variable, but numerous stream sections, notably the Iaen and Dugoed, provide excellent exposures of many of the fold and cleavage structures.

An analysis of the trends of the major and minor folds of the district suggests that there are three fold systems developed: most of the structures trend approximately N.N.E.-S.S.W., some nearly E.-W. and a few N.W.-S.E. The N.N.E.-S.S.W. system is characterised by parallel folds which are all asymmetrical and sometimes slightly overturned. The great majority of the folds have their axial planes inclined westwards, but in the extreme north-west of the area, near Mallwyd, two folds slightly overturned to the west are exposed ; while in the Clegyr Valley near the centre of the Talerddig area there is a series of small folds with axial planes inclined westwards which are believed to be drag folds associated with the northward continuation of the fault zone developed in the Twymyn valley of the Tarannon area. The folds trending predominantly N.N.E.-S.S.W. exhibit an arcuate form—in the south of the area most of the folds trend N.E.-S.W., in the centre they trend N.-S., and in the north they trend N.N.E.-S.S.W. The majority of the folds are periclinal although a few well-formed basins are developed. In addition to the E.-W. and E.S.E.-W.N.W. culminations and depressions associated with the predominantly N.N.E.-S.S.W. folds, a few distinct E.W. folds also occur. They are parallel folds either asymmetrical or slightly overturned to the south, absent from the western part of the area. A small number of folds, confined to the western part of the district cannot be attributed to either N.N.E.-S.S.W. or E.-W. fold systems for they trend N.W.-S.E. They are parallel asymmetrical folds with their axial planes inclined north-eastwards.

Cleavage planes are very well developed throughout the area. They usually appear to be fracture cleavage, and form small fans radiating outwards from the core of each fold. A consideration of the cleavage trends suggests that two sets are developed. The first set, developed throughout the area, is associated with the N.N.E.-S.S.W. folds and since the cleavage planes are aligned parallel to the trend of the folds they exhibit an arcuate form. The second is developed on the limbs of the E.-W. folds, is aligned parallel to the axial line of the folds and is found only in those localities where E.-W. folds occur. Along the western slopes of Mynydd Plas, near the centre of the area, the cleavage planes exhibit every gradation in direction when traced northwards—from N.N.E.-S.S.W. cleavage associated with a N.N.E.-.S.S.W. fold in Nant Carfan, through N.-S. and N.W.-S.E. cleavage to E.-W. cleavage associated with the E.-W. fold in Nant Tafolog. Although cleavage planes trending N.W.-S.E. occur in this locality, they are not developed elsewhere and do not appear to be associated with the N.W.-S.E. folds.

The fold and cleavage patterns in the Talerddig District appear to be composite in origin, their complexity arising from their geographical setting between four major domes, three of which, the Harlech dome to the north-west, the Plynlimon dome to the south-west, and the Van dome to the south have a structural trend between N.N.E.-S.S.W. and N.-S., while the fourth, the Berwyn dome to the north-east, has in places an E.-W. trend. The main fold system in the Talerddig area, the N.N.E.-S.S.W. one, is associated with the folds of a similar trend and symmetry to the west and south. The smaller number of E.-W. folds, having no equivalents to the west, north or south, are presumably

associated with the folds of similar trend and asymmetry to the east and north-east. These E.-W. folds are fewer in number than the N.N.E.-S.S.W. folds because the Berwyn dome is slightly further away than the other three domes. Folds with a N.W.-S.E. trend do not occur in any of the areas adjoining the Talerddig District. It is doubtful if any correlation can be made between the N.W.-S.E. folds of the Talerddig District and those which occur to the north of the Bala fault in the Bala district.

The various directions of cleavage show a similar geographical distribution to the folds : all the cleavage planes immediately west, south and north of the Talerddig area trend either N.N.E.-S.S.W. or N.-S., while the E.-W. trending cleavage planes are confined to the eastern part of the Talerddig area and districts to the east and north-east.

Gravity and Magnetic Anomalies in North Wales

by

Dr. D. W. Powell

In 1952 and 1953 a gravity survey of North Wales was made, the south-eastern boundary being approximately a line from Aberystwyth to Chester. A Nörgaard gravimeter was used. Areas which showed interesting gravity anomalies were then surveyed with a magnetometer, the additional data obtained being helpful in interpretation.

In the north-east part of the region surveyed the anomalies are due to the Upper Palaozoic and the Triassic, whereas elsewhere they are related to older rocks. In the north-east the vale of Clwyd is the most interesting area. It was found to be a well-defined gravity low divided into two by a ridge near St. Asaph, this corresponding to an outcrop of Coal Measures. In the northern part, near Rhyl, there is known to be 100 ft. of Drift and 500 ft. of Trias, underlain by an unknown thickness of presumed Upper Carboniferous rocks, these overlying the Carboniferous Limestone. Using this and other geological information, calculation from the measured anomaly gives a maximum thickness of 2,500 ft. for the Upper Carboniferous rocks. Lack of geological information makes a similar calculation for the southern basin impossible, but if it is assumed that no Upper Carboniferous is present south of St. Asaph and that the Trias lies directly on Carboniferous Limestone, the calculated thickness for the Trias in this area is 2,000 ft. Even if this assumption is not true the calculation at least suggests that the Trias is much thicker here than to the north. The gravity data are consistent with the boundaries of the vale being either faults or scarps.

Somewhat similar calculations give a thickness of 2,500 ft. for the Upper Carboniferous at Point of Ayr.

In the south-west the most striking features of the survey are the gravity and magnetic lows on the coast between Harlech and Barmouth. Both take the form of a zone of steep gradients parallel to the coast, and both appear to swing round to the north and continue to follow the coast to St. Tudwal's. Inland the gravity and magnetic fields rise to a high over the Dolwen pericline of the Harlech dome. Calculations suggest the pre-Cambrian may lie here at 2,000 ft. below the surface. Of considerable significance appears to be the gradual increase of anomaly from + 10 mgls. in the south-east to over + 40 mgls. at Menai in the north-west, and its decrease to + 25 mgls. over Anglesey. These regional changes are difficult to explain in terms of surface geology, and their source may perhaps be found in the structure of the crust.

Using normally accepted values of thicknesses and densities for the crustal layers a satisfactory fit to the gravity data is obtained with a crustal upwarp which increases from zero at Malvern to 2,000 ft. at Bala, increases again to something like 12,000 ft. at Menai, and then decreases rapidly towards Holyhead. There is some support for such an explanation in the geological evidence of post-Cretaceous uplift in Wales.

SECTION H.—ANTHROPOLOGY

EXPLORATION OF CAVES IN THE DERBYSHIRE DISTRICT

INTERIM REPORT No. 25 (1953) of COMMITTEE appointed to co-operate with a Committee of the Royal Anthropological Institute in the exploration of caves in the Derbyshire district. Mr. M. C. BURKITT, *Chairman*; Mr. A. LESLIE ARMSTRONG, *Secretary*; Dr. ARTHUR COURT, Prof. H. J. FLEURE, F.R.S., Mr. W. H. HANBURY, Dr. J. WILFRED JACKSON, Mr. R. U. SAYCE.

During the past year the excavation of Ash Tree Cave, Burntfield Grips, near Whitwell, N.E. Derbyshire, has been continued by Mr. Leslie Armstrong, Dr. Arthur Court, Mr. W. H. Hanbury and Mr. Horace Jones.

Owing to the burden of increasing years and the heavy work involved by the nature of the excavation, participation in actual digging by Dr. Court and Mr. Hanbury has been limited and their efforts mainly confined to the sorting and cleaning of sieved material. Invaluable assistance has again been rendered by Mr. Horace Jones in the arduous work of excavation. Work in the cave is in active progress and the examination of *Site No.* 2, on the N.E. side of the valley, opposite to Ash Tree Cave, has been completed.

Mr. Leslie Armstrong reports that *Site No.* 2 has proved somewhat disappointing, as it was found to be a small rock shelter and not a cave. The L-shaped exploratory trench—one arm parallel to the face and the other at right angles thereto—was sunk through the glacial deposit to a depth of 5 feet, and that which sealed the entrance to what appeared to be a cave was entirely removed. This revealed not a cave, but a low vertical rock wall 4 feet beyond the face of the crag, towards which the overhanging portion of the crag recessed backwards in a series of steps resulting from the fall of successive layers of the tabular limestone. The base was an irregular platform of similar disintegrated rock and the extreme height of the shelter thus formed was 5 feet. Both arms of the trial trench were sunk to a depth of 5 feet, and penetrated through the glacial deposit to its base. The deposit was 4 feet 6 inches in thickness, consisting of a brown calcareous loam containing fragments of the local limestone, some of which were considerably abraded; also numerous erratics. The erratics included Bunter pebbles; pebbles of red and white quartz; fragments of Carboniferous limestone, considerably abraded; Lake District rocks; an abraded pebble of basalt and an extremely hard piece of coarse grit-stone, almost a conglomerate, probably of Northern origin. This glacial deposit rested upon a bed of yellowish calcareous loam, almost free from stones, of which only 6 inches was examined, but it appeared to be similar to the bed of loam noted at the base of the glacial deposit in the section cut outside the Ash Tree Cave and probably represents an earlier glacial deposit which was subjected to weathering, or solifluxion. No artifacts or evidence of human occupation were found beneath the glacial material, but small flecks of charcoal were noticeable in several places on the line of junction with the base bed, which might be indicative of casual occupation by man.

Ash Tree Cave.—Work has been confined to the central cross-section of the cave which when the 1952 Report was written had reached a depth, at one end, of 9 feet 6 inches. The width of the section has been increased to 7 feet 6 inches, and the depth, over 8 feet of the length, sunk to 10 feet 6 inches below datum level. Progress has been slow owing to the large slabs of rock encountered, but evidence of the division into three Zones of Mousterian occupation, as set out in the previous report, has been strengthened and valuable evidence of climatic conditions obtained. Reference was made in the last

report to the occurrence in Zone 2 of a human tooth which, though found in association with Mousterian artifacts was believed to be that of a Neanthropic woman and probably introduced from an upper level into Zone 2. Confirmation of this assumption, and the probable method by which the introduction was effected, has been obtained during the excavation of an adjoining portion of Zone 2, in which was found a Creswellian flint flake and part of a barbed bone spear, of the type found in the lowest Creswellian horizon of Whaley Cave. These objects were found in a small space separating two large slabs of rock, each of which were deeply embedded in the cave earth and inclined at steep angles, approaching 60 degrees. The largest measured 2 feet × 1 foot 6 inches × 6 inches ; the other was a little smaller ; each rested with the heaviest end downwards. It seems clear that the slabs had fallen from the roof when the cave earth was in a water-logged, or sludged, condition and in so doing had carried down into the Mousterian Zone these Upper Palaeolithic artifacts. If this assumption is correct, it would appear that the physical conditions indicated are to be linked with the introduction into the cave of the boulder clay which sealed the Creswellian occupation level.

A layer of the Mousterian Zone 3, 1 foot in thickness, has been excavated over three-quarters of the area of the section, from 9 feet 6 inches to 10 feet 6 inches below datum level. It exhibits much less evidence of disturbance, and the contained stones, though numerous, are less massive. In general character it resembles the upper level layers, but has proved more productive of artifacts and animal remains, particularly teeth, amongst which teeth of Cave Bear and Cave Lion are noticeable, also a tooth of Giant Deer, together with specimens of all the fauna previously recorded. Interesting fragments of an, as yet, unidentified species of deer have been recovered. Fragments of bone, both gnawed and humanly split, are frequent. The artifacts include split and chipped quartzite pebbles ; pebble pounders ; utilised split bones ; bone tools and a bone awl. The bottom of this occupation zone has not yet been reached.

M. C. Burkitt, *Chairman.*
August, 1953. A. L. Armstrong, *Secretary.*

SEISMOLOGICAL INVESTIGATIONS

FIFTY-EIGHTH Report of the Committee on Seismological Investigations. (Dr. R. Stoneley, F.R.S., *Chairman* ; Mr. E. Tillotson, *Secretary* ; Mr. E. F. Baxter, Miss E. F. Bellamy, Prof. P. G. H. Boswell, O.B.E., F.R.S., Mr. B. C. Browne, Sir E. C. Bullard, F.R.S., Dr. K. F. Chackett, Dr. G. E. R. Deacon, F.R.S., Dr. A. T. J. Dollar, Dr. A. E. M. Geddes, O.B.E., Prof. G. R. Goldsbrough, C.B.E., F.R.S., Dr. M. N. Hill, Mr. J. S. Hughes, Prof. Sir H. Jeffreys, F.R.S., Mr. E. N. Lawrence, Dr. A. W. Lee, Dr. A. O. Rankine, O.B.E., F.R.S., Rev. C. Rey, S.J., Dr. G. D. Robinson, Mr. H. V. Shaw and Sir Frank Smith, G.C.B., G.B.E., F.R.S.)

OBITUARY

The committee records with deep regret the death on March 10, 1953, of the Rev. H. N. Edge, who had been a member of the committee since 1950. After a scientific training Mr. H. N. Edge was employed at the Meteorological Office where he became interested in seismology, an interest which he held to the last. Lately he was vicar of Walton in Cumberland and he had only recently retired.

PERSONAL

The committee offers its hearty congratulations to Prof. Sir Harold Jeffreys, F.R.S., and Sir Edward Bullard, F.R.S.,

whose knighthoods were announced in the New Year Honours and the Coronation Honours respectively.

THE GRAY-MILNE FUND

Apart from secretarial expenses, there have been no disbursements from the fund during the past year. The sum of £400 still remains invested in Defence Bonds.

GRAY-MILNE FUND

Receipts	£	*s.*	*d.*
Balance at Bank, July 1, 1952,	370	8	2
Trust Income . . .	80	14	10
Interest on Defence Bonds .	10	0	0
	461	3	0

Payments	£	*s.*	*d.*
Secretarial Expenses . .	5	5	3
Balance at Bank, June 30, 1953	455	17	9
	461	3	0

INSTRUMENTS

There has been no change in the location of the instruments belonging to the committee during the past year. The instruments are located as follows :—

Milne-Shaw Seismographs,	
No. 1 . . .	Science Museum, South Kensington.
„ 3 . . .	Edinburgh.
„ 4 . . .	Downe, Kent (in storage).
„ 6 . . .	Hermanus, South Africa.
„ 27 . .	Perth, Western Australia.
„ 63 . . .	Suva, Fiji.
Seconds Regulator Clock . . .	Suva, Fiji.
Jaggar Shock Recorder	Fort Augustus.

The Director of the New Zealand Meteorological Service reports that the Seconds Regulator Clock at Suva, Fiji, having been frequently repaired, now appears to be beyond further repair and a replacement is required.

THE INTERNATIONAL SEISMOLOGICAL SUMMARY

By J. S. Hughes

There have been changes on the staff of the Summary during the period covered by the present report. Mr. B. Elliott left in May to take his National Service training, and there remain four assistants on the work. Mrs. Saunders continues to take charge of the clerical and secretarial side, assisted by Miss P. Marlow, Miss V. Howe, who came in September 1952, continues on the preparation of manuscript, and during the first three months of 1953 temporary assistance was given by Dr. R. I. B. Cooper of the Department of Geodesy and Geophysics at Cambridge. Mr. J. H. McDonnell of St. John's College, Cambridge, will be joining the staff shortly though he still remains subject to National Service obligations.

Progress has been satisfactory during the past year and the second quarter of the I.S.S. for 1943 will be issued soon. In the report for 1952 it was stated that the third part of 1941 was expected so that in the period covered seven quarterly numbers are accounted for. Little diminution in the size of the volume has taken place as yet, but it is expected that 1944 will be a light year. Important groups of station reports fail for this period, though they come in again later.

DEPARTMENT OF GEODESY AND GEOPHYSICS, CAMBRIDGE

EXPERIMENTAL.—During the summer of 1952, seismic refraction shooting experiments with sono-radio buoys were carried out by the Department of Geodesy and Geophysics from R.R.S. *Discovery II* in collaboration with H.M.S. *Scott*. These experiments were confined to the deep water of the N.E. Atlantic and to the continental area of the Western Approaches. From the longer range experiments the depth of the Mohorovčić discontinuity was determined. It lies at a depth of approximately 11 km. below the sea level in this area ; this value agrees with the American measurements made in the Western Atlantic and in the Eastern Pacific. The sedimentary thickness was found to be variable over a comparatively

wide range but had a mean value of approximately 1·5 km.; this layer overlies the layer found extensively under the oceans in which the velocity is 6·3 km./sec.

The world cruise of H.M.S. *Challenger* was completed in the autumn and the many seismic refraction shooting results are being worked out. It appears that the typical deep sea structure in all oceans is similar to that described above. In deep sea areas near sea mounts or volcanic islands there is, however, an additional layer below the sediments representing basaltic lava flows in which the velocity of longitudinal waves is about 4·8 km./sec. This layer was particularly extensive in the Indian Ocean.

During the summer of 1953 the Department of Geodesy and Geophysics are making further experiments with sono-radio buoys from H.M.S. *Challenger* in the eastern Atlantic. These experiments extend out to the eastern flanks of the mid-Atlantic Ridge.

THEORETICAL.—The work on the propagation of Rayleigh waves in a double surface layer is being continued. For the special case in which the granitic and basaltic layers are assumed to be of equal thickness the variation of wave velocity and group velocity with wave length has been tabulated by the Mathematical Laboratory; as an experiment these values were worked out by hand machine. It is intended to complete the work by use of the electronic computer (EDSAC). The values so far found have confirmed those obtained by the Rayleigh approximation, which, of course, gives an overestimate of the velocity.

THE NATIONAL INSTITUTE OF OCEANOGRAPHY

By G. E. R. Deacon

Mr. Darbyshire, by analysing records from Kew, has obtained further evidence that microseismic waves are mixtures of Rayleigh and Love waves, and that it is possible to separate the two by cross-correlation of recordings of the vertical and horizontal components. Having separated them, it is possible to find the direction of approach of the Rayleigh waves, and in the examples chosen this agreed closely with the bearing of the region of wave interference below which the microseismic waves were generated. A preliminary account of the work was given at a Geophysical Discussion arranged by the Royal Astronomical Society and it is being prepared for publication.

RECENT GREEK EARTHQUAKES

By E. Tillotson

The provisional readings of the Edinburgh E. W. Milne-Shaw seismograms for the three principal shocks of the recent swarm of earthquakes destructive in the Ionian islands of Ithaca, Cephalonia and Zante are in G.M.T. as follows: 1953, August 9: eP 07h. 46m. 26s., PPP 07h. 47m. 15s., S 07h. 50m. 45s., SS 07h. 51m., 41s., M 07h. 56m. 17s., F 08h. 28m.

1953, August 11: P 03h. 37m. 41s., PP 03h. 38m. 15s., PPP 03h. 38m. 28s., S 03h. 42m. 01s., M 03h. 46m. 53s., F 04h. 30m.

1953, August 12: P 09h. 29m. 12s., S 09h. 33m. 31s., M 09h. 40m., F 10h. 40m.

The last shock exhibited the greatest amplitudes. In all three shocks the initial P wave had, compared with the majority of earthquake records from Edinburgh, comparatively large amplitudes. This was particularly so on August 11.

HISTORICAL

By E. Tillotson

The recent destructive earthquake swarm in the Ionian islands recalls some early investigations by British Seismologists, using Greek earthquakes.

1. Dr. C. Davison in his *Origin of Earthquakes* (C.U.P.), p. 20, writes: 'For instance, 143 shocks and earth-sounds were noted at Comrie in Perthshire during the last three months of 1839; 306 shocks were felt in the island of Zante during the year 1896; while at Gifu in Japan, 3,365 shocks were recorded between October 28, 1891, and the end of the year 1893. There are reasons for connecting the earthquakes at all three places with neighbouring faults, and it is quite inconceivable that each shock was caused by the formation of a new fracture or the

extension of an old one.' The inference is that successive slipping occurred along one fault, i.e. the same fault was extended in depth but not laterally.

2. Prof. H. H. Turner in the introduction to the I.S.S. for 1925 January, February, March (p. 6), writes : ' The opportunity was taken to review the earthquake history of S.E. Europe, during the seven years for which the I.S.S. has been published (1918–24) and it appears that there is a line of epicentres running from about 45° N. 10° E. to 38° N. 30° E. . . . it will be said that Chirpan (25°. 4 E. 42°. 3N.) now lies pretty close to the line though the trend of it was settled from previous observations. Corinth (22°. 9 E . 37°. 9N.) is also not far from this line.

About 5° south of this wavy line is another running roughly parallel with it, on which at least 36 epicentres lie, according to present observations. But to test this rough assignment thoroughly would require a good deal of time and labour which cannot at present be spared.'

3. In the British Association Seismological Committee's second report to Section A in 1897 Mr. J. Milne contributes a section on the fracture of submarine telegraph cables, which states : ' The Zante-Canea interruption (March 29, 1885), occurred about 5 miles west by south of Sapienza Island, in a depth of 1,500 fathoms with a clay bottom. Soundings varied as much as 250 fathoms in the length of the ship, and from 1,350 to 1,834 fathoms in half a mile. . . . In 1867, after the destruction of Cephalonia, the soundings taken after the shock were different from those taken before. Again, on September 9, 1888, at 5.4 P.M., the town of Vostizza, in the Gulf of Corinth, was destroyed, and simultaneously the cable between Zante Patras and Corinth was interrupted. The cause of this, as deduced from soundings and the appearance of the fractured cable, appears to have been either a sudden tautening caused by the sweeping down of a mass of clay from a 100-fathom bank to a 300-fathom bank, or the actual yielding of the bed on which the cable lay. . . . The facts are that the sub-oceanic contours are such that they might be expected to be unstable, and that these contours, at the time of earthquakes, have suddenly been changed. In one instance there has been an increase in depth of over 2,400 ft., and in another 1,200 ft. ; while in the case of the 1889 disturbance, 11½ minutes later, unfelt earth waves of considerable magnitude were recorded at Wilhelmshaven, 1,732 kilometres distant. Similar unfelt movements have also been recorded at distant places at about the time when cable-interruptions took place, in every instance where we have been able to make comparisons. The conclusion, then, is that in this region earthquakes occur, producing beneath the ocean what is equivalent to the landslips which similar movements produce on land.'

4. The possible relationship between some magnetic and some seismic phenomena was discussed by Prof. H. H. Turner in the B.A. Seismological Committee's 34th Report to Section A in 1929 : ' An earthquake with date 1928, April 22d. 20h. 13m. 50s., which partly destroyed the city of Corinth, was mentioned in the last report. The position of Corinth is 37°.9N., 22°.9e. ; that of the epicentre (according to readings received from Helwan, San Fernando, Toledo, Tortosa, Paris and Vienna) is not far from 40°.0N., 23°.0E. By the kindness of the hydrographer an interesting note has been received of a disturbance of the magnetic compass on H.M.S. *Argus* in 33° 38′ N., 24° 4′ E., sailing on an easterly course (286° true or N71° W. standard). In reporting the incident Commander Faulkner of H.M.S. *Argus* writes : " As both magnetic compasses were deflected similarly, it is doubtful whether the vessel not fitted with gyrocompasses would have been aware of the disturbance. Unfortunately no azimuths could be taken during the phenomenon. . . ." The duration of the perturbation is noteworthy. At 18.20 G.M.T. the difference was 5° ; after 10 minutes the difference began to decrease until 19.10 G.M.T. the standard compass course became normal again—to be followed shortly after by the steering compass. . . . Prof. S. Chapman is investigating the matter from a magnetic point of view.'

The same problems can still be examined with advantage to-day, with our improved apparatus.

Prof. B. Gutenberg of Pasadena is

currently of the opinion that in general many of the Greek earthquakes are of intermediate focal depth (70–300 km.) and associated with volcanic phenomena.

INTERNATIONAL

Prof. Sir Harold Jeffreys, F.R.S., Dr. R. Stoneley, F.R.S., and Mr. E. Tillotson attended the meetings of the European Seismological Commission held in Stuttgart from September 22 to 27, 1952. There were present people interested in seismology from fourteen countries in Europe and Africa. Very interesting and informative discussions took place, and it seems worth while to repeat here the text of five resolutions adopted at the end of the meeting (I.U.G.G. *News Letter*, 2nd year, No. 2, April 1953, published by the Secretary-General of the I.U.G.G., G. Laclavère of Paris, and financed by U.N.E.S.C.O.).

Resolution 1 : Noting that the only certain method for the precise determination of the depths of foci requires the use of seismograms obtained very near to the epicentres, the European Seismological Commission recommends the setting up of new seismological stations equipped with modern apparatus permitting the determination of arrival times with an accuracy at least to one-tenth of a second, the stations to be sited in the immediate vicinity of active seismic zones.

Resolution 2 : The European Seismological Commission recommends the establishment of national catalogues which shall include for each earthquake : (1) The geographical co-ordinates of the epicentre and the precision with which these co-ordinates have been determined. (2) The maximum macroseismic intensity observed. (3) The macroseismic area in square kilometres. (4) For twentieth-century earthquakes an indication of the magnitude of the earthquake, or failing this, the class according to Gutenberg's definition (classes d, e) ; a class f may be used to indicate an earthquake registered at one near station only. In each country an attempt should be made to establish a relationship between magnitude defined according to Gutenberg and Richter scale and the microseismic and macroseismic data.

Resolution 3 : Noting that one may expect a better acquaintance of the structure of the European continent by the recording of reflections upon the deep layers of the earth's crust, the European Seismological Commission recommends that experiments using large explosive charges be arranged and carried out in particularly interesting regions, for example the region of negative gravity anomalies in the Alps, the Hohe Tauern, the region of magnetic anomalies in the Paris Basin, etc. A total expenditure of the order of 300,000 dollars would be needed for a dozen explosions ; the European Seismological Commission asks governments to contribute to this study.

Resolution 4 : The European Seismological Commission recommends the institution of a service for the rapid determination of the epicentres of earthquakes taking place in Europe and neighbouring regions. Each time that an important near earthquake is recorded at an epicentral distance of less than 5,000 km., European and circum - Mediterranean stations are invited to send by the quickest means (Telegram, Air Mail) to the Bureau Central International de Sismologie at Strasbourg the provisional readings of their seismograms or at least the exact time, accurate to one-tenth of a second if possible, of the first impulse. The bureau will send out as quickly as possible the epicentral determination.

Resolution 5 : The European Seismological Commission strongly advises seismological stations not to destroy their old seismograms, even if these do not seem to have anything of current interest on them. They should take care to preserve their collection intact.

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